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OBSERVATIONS

ON

CERTAIN PARTS

OF THE

ANIMAL OECONOMY.

By JOHN HUNTER.

SECOND EDITION.

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MDCCXCII.

TO

SIR JOSEPH BANKS, Bart.

PRESIDENT OF THE ROYAL SOCIETY,

&c. &c. &c.

DEAR SIR,

As the following Obſervations were made in the Courſe of thoſe Purſuits in which you have ſo warmly intereſted yourſelf, and promoted with the moſt friendly Aſſiſtance, I ſhould be wanting in Gratitude, were I not to addreſs them to you, as a public Teſtimony of the Friendſhip and Eſteem with which I am,

DEAR SIR,

YOUR OBLIGED, AND

VERY HUMBLE SERVANT,

JOHN HUNTER.

LEICESTER-SQUARE,
NOVEMBER, 9, 1786.

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A DESCRIPTION OF THE SITUATION OF THE TESTIS IN THE FŒTUS, WITH ITS DESCENT INTO THE SCROTUM.

A Diſcovery in any art, not only enriches that with which it is immediately connected, but elucidates all thoſe to which it has any relation. The knowledge of the conſtruction of a human body is eſſential to medicine, therefore every improvement in anatomy muſt throw additional light on that branch of ſcience. Theſe improvements ſtrike more forcibly, if they are on ſubjects quite new or little underſtood; and this effect is well illuſtrated by the advantages which pathology has derived from the diſcovery of the lymphatics being the abſorbent ſyſtem; and likewiſe by that caſe of hernia, where the inteſtine lies in contact with the teſticle; which has been perfectly explained by the diſcovery of the original ſeat of the teſticle being in the abdomen.

Several years before Haller's Opuſcula Pathologica were publiſhed, my brother informed me, that in examining the contents of the abdomen of a child, ſtillborn, about the ſeventh or eighth month, he found both the teſticles lying in that cavity, and mentioned the obſervation with ſome degree of ſurprize. By this we are enabled to account for a circumſtance that ſometimes happens in the ſcrotal hernia, as depending on the diſcovery that, the teſtis is formed in the abdomen; and which we could never explain to our ſatisfaction, till the publication of the Opuſcula, to which Dr. Hunter alludes, Commentaries, page 72, in the following words:

"In the latter end of the year 1775, when I firſt had the pleaſure of reading Baron Haller's Obſervations on the Hernia Congenita*, it ſtruck my imagination that, the ſtate of the teſtis in the fœtus, and its deſcent from the abdomen into the ſcrotum, would explain ſeveral things concerning ruptures and the hydrocele, particularly that obſervation which Mr.

* Alberti Halleri Opuſcul. Patholog. Lauſan. 1755, 8vo. page 53, &c.

B

Sharp had communicated to me, viz. That in ruptures the inteſtine is ſometimes in contact with the teſtis. I communicated my ideas upon this ſubject to my brother, and deſired that he would take every opportunity of learning exactly the ſtate of the teſtis before and after birth, and the ſtate of ruptures in children. We were both convinced that the examination of thoſe facts would anſwer our expectation, and both recollected having ſeen appearances in children that agreed with our ſuppoſition; but ſaw now that we had neglected making the proper uſe of them.

"In the courſe of the winter, my brother had ſeveral opportunities of diſſecting fœtuſes of different ages, and of making ſome drawings of the parts; and all his obſervations agreed with the ideas I had formed of the nature of ruptures, and of the origin of the tunica vaginalis propria in the fœtus. But till thoſe obſervations were repeated to his ſatisfaction, and were ſufficiently aſcertained, he deſired me not to mention the opinion in my lecture; and therefore, when treating of the coats of the teſtis, and of the ſituation of the hernial ſac, &c. I only put in this temporary caution, that I was then ſpeaking of thoſe things as they are commonly in adult bodies, and not as they are in the fœtus: and at laſt, when I was concluding my lectures for that ſeaſon, in the end of April 1756, with a courſe of the chirurgical operations, I gave a very general account of my brother's obſervations, and ſhewed both the drawing of Fig. II. which was then finiſhed, and the ſubject from which it was made."

The following obſervations on this ſubject were taken from my notes, and publiſhed by Dr. Hunter, in his Commentaries, to which I have added ſome practical remarks.

Until the approach of birth, the teſtes of the fœtus are lodged within the cavity of the abdomen, and may therefore be reckoned among the abdominal viſcera. They are ſituated immediately below the kidneys, on the forepart of the pſoæ muſcles, and by the ſide of the rectum, where this inteſtine is paſſing down into the cavity of the pelvis: for in the fœtus, the rectum, which is much larger in proportion to the capacity of the pelvis than in the fullgrown ſubject, lies before the vertebræ lumborum as well as before the os ſacrum. Indeed the caſe is pretty much the ſame with regard to all the contents of the pelvis; that is, their ſituation is much higher in

the fœtus than in the adult. The ſigmoide flexure of the colon, part of the rectum, the greateſt part of the bladder, the fundus uteri, the Fallopian tubes, &c. being placed in the fœtus above the hollow of the pelvis in the common or great abdominal cavity.

While the teſtis remains in the abdomen, its ſhape or figure is much the ſame as in the adult; and its poſition or attitude the ſame as when it is in the ſcrotum: that is, one end is placed upwards, the other downwards; one flat ſide is to the right, the other to the left; and one edge is turned backwards, the other forwards, and the veſſels enter the poſterior edge alike in both the fœtus and adult. As the teſtis is not ſo immediately incloſed in the ſurrounding parts while it is in the loins, its poſition may be a little variable; and the moſt natural ſeems to be when the anterior edge is turned directly forwards; but as the leaſt touch of any thing will throw that edge either to the right ſide, or to the left, then the flat ſide of the teſtis will be turned forwards. It is attached to the pſoas muſcle all along its poſterior edge, except juſt at its upper extremity; and this attachment is formed by the peritonæum, which covers the teſtis and gives it a ſmooth ſurface, in the ſame manner as it envelopes the other looſe abdominal viſcera.

The epididymis lies along the outſide of the poſterior edge of the teſtis, as when in the ſcrotum, but is larger in proportion, and adheres backwards to the pſoas. When the fœtus is very young, the adheſion of the teſtis and epididymis to the pſoas is very narrow; and then the teſtis is more looſe, and more projecting: but as the fœtus advances in months, the adheſion of the teſtis to the pſoas becomes broader and tighter.

The veſſels of the teſtis, like thoſe of moſt parts of the body, commonly riſe from the neareſt larger trunks, viz. from the aorta and cava, or from the emulgents.

The artery generally riſes from the forepart of the aorta, a little below the emulgent artery, and often from the emulgent itſelf, eſpecially in the right ſide of the body, which may happen the rather, becauſe the trunk of the aorta is more diſtant from the right teſtis than from the left. Sometimes, but much more rarely, the ſpermatic artery ſprings from the phrenic, or from that of the capſula renalis. Beſides the artery which

riſes from the aorta, or emulgent, &c. the teſtis receives one from the hypogaſtric artery, which is ſometimes as large as the other. It runs upwards from its origin, paſſing cloſe to the vas deferens in its way to the teſtis. The ſuperior ſpermatic artery ſometimes paſſes before the lower end of the kidney; and both theſe arteries run in a ſerpentine direction, making pretty large but gentle turnings. They are ſituated behind the peritonæum; and both run into the poſterior edge of the teſtis, between the two reflected laminæ of that membrane, much in the ſame manner as the veſſels paſs to the inteſtines between the two reflected laminæ of the meſocolon or meſentery.

The veins of the teſtis are analogous to its arteries; but commonly change ſides with the arteries reſpecting their origins from the emulgents. The ſuperior ſpermatic vein, to begin with its trunk, riſes commonly in the following manner: on the right ſide, from the trunk of the vena cava, a little below the emulgent, and on the left ſide from the left emulgent vein. The reaſon of this difference between the right and left ſpermatic vein, no doubt, is becauſe the cava is not placed in the middle of the body; ſo that by the rule of ramification, which is obſerved in moſt parts of the body, the cava is the neareſt large vein of the right ſide, and the emulgent is the neareſt large vein of the left ſide. But the difference is inconſiderable; and accordingly we ſometimes find the right ſpermatic vein coming from the right emulgent vein; and ſeveral other varieties are produced, which, ſo far as I can obſerve, follow no preciſe rule. There is likewiſe a ſpermatic vein, which riſes from the internal iliac, and runs up to the teſtis with the inferior ſpermatic artery. Both the ſpermatic veins run behind the peritonæum with their correſponding arteries, and go into the poſterior edge of the teſtis, where they are loſt in ſmall branches.

The nerves of the teſtis, like its blood-veſſels, come from the neareſt ſource; that is, from the abdominal plexuſes of the intercoſtal; eſpecially the inferior meſenteric plexus. They run to the teſtis, accompanying its blood-veſſels, and are diſperſed with them through its ſubſtance. The teſtis therefore, with reſpect to its nerves, may be reckoned an abdominal viſcus; and this obſervation will hold good, when applied to the full-grown ſubject, as well as to the fœtus; for thoſe branches of the lumbar

nerves, which are commonly ſaid to be ſent to the teſtis, paſſing through the tendon of the external oblique muſcle, in reality go not to the teſtis itſelf, but to its exterior coverings, and to the ſcrotum.

The teſticle receiving its nerves from the plexuſes of the intercoſtal, accounts for the ſtomach and inteſtines ſympathiſing ſo readily with it and its particular ſenſation, and for the effects ariſing in the conſtitution upon its being injured.

The epididymis begins at the outer and poſterior part of the upper end of the teſtis, immediately above the entrance of the blood-veſſels, where it is thick, round, and united to the teſtis. As it paſſes down, it becomes a little ſmaller and more flat, and is only attached backwards to the teſtis, or rather indeed to its veſſels; for its anterior edge lies looſe againſt the ſide of the teſtis forwards; and at its lower end it is again more firmly attached to the body of the teſtis, ſo that in the fœtus there is a cavity or pouch formed between the middle part of the teſtis, and the middle part of the epididymis, more conſiderable than is commonly obſerved in fullgrown ſubjects. As the body grows, the epididymis adheres more cloſely to the ſide of the teſtis; and its greateſt part is made up of one convoluted canal, which becomes larger in ſize and leſs convoluted towards the lower end, and at laſt is manifeſtly a ſingle tube running a little ſerpentine. That change happens at the lower end of the teſtis, and there the canal takes the name of vas deferens.

The vas deferens is a little convoluted, or ſerpentine, in its whole courſe, but is leſs ſo as it comes nearer to the bladder; inſtead of running upwards from the lower end of the teſtis, as it does when the teſticle is in the ſcrotum, while that remains in the abdomen, it runs downwards and inwards in its whole courſe, ſo that it goes on almoſt in the direction of the epididymis, of which it is a continuation. It turns inwards from the lower end of the epididymis, under the lower end of the teſtis, and behind the upper end of a ligament or gubernaculum teſtis, (which I ſhall preſently deſcribe) then it paſſes over the iliac veſſels and over the inſide of the pſoas muſcle, ſomewhat higher than in adult bodies; and at laſt goes between the ureter and bladder towards the baſis of the proſtate gland.

In thoſe animals where the teſticles change their ſituation, the cremaſter muſcle, which ſhould be named muſculus teſtis, has two very different poſitions in the fœtus, and in the adult: the firſt, being the ſame as in thoſe animals whoſe teſticles remain through life in the cavity of the abdomen; we muſt therefore conclude that, the ſame purpoſes are anſwered by this muſcle in the fœtus, as in thoſe animals.

The uſe of this muſcle, when the teſticle is in the ſcrotum, appears to be evidently that of a ſuſpenſory; for I find this muſcle is ſtrong in proportion to the ſize of the teſticle, and pendulous ſituation in other animals. But what purpoſe it anſwers in the fœtus, or in animals whoſe teſticles remain in the abdomen, is not eaſily imagined, there being no apparent reaſon why ſuch a muſcle ſhould exiſt.

The cremaſter, or muſculus teſtis, appears to be compoſed of the lower fibres of the obliquus internus and tranſverſalis muſcles in the fœtus, turning upwards, and ſpreading upon the anterior ſurface of the gubernaculum, immediately under the peritonæum; it appears to be loſt on the peritonæum, a little way from the teſticle. This, although now inverted, is more evidently ſeen in adult ſubjects, who have had a hydrocele, or rupture; in ſuch caſes the muſcle becomes ſtronger than uſual, and its fibres can be traced ſpreading on the tunica vaginalis, and ſeem at laſt to be loſt upon it, near to the lower end of the body of the teſticle.

The nerves which ſupply this muſcle are, probably, branches from the nerves of the obliquus internus and tranſverſalis muſcles; for the ſame cauſe which throws the abdominal muſcles into action, produces a ſimilar effect on the muſculus teſtis; which circumſtance appears to be moſt remarkable in the young ſubject. When we cough or act with the abdominal muſcles, we find the teſticles to be drawn up; the muſculus teſtis and abdominal muſcles taking on the ſame action from the ſame cauſe.

At this time of life the teſtis is connected in a very particular manner with the parietes of the abdomen, at that place where in adult bodies the ſpermatic veſſels paſs out, and likewiſe with the ſcrotum. This connection is by means of a ſubſtance which runs down from the lower end of the teſtis to the ſcrotum, and which at preſent I ſhall call the ligament, or gubernaculum teſtis, becauſe it connects the teſtis with the ſcrotum,

and ſeems to direct its courſe through the rings of the abdominal muſcles. It is of a pyramidal form; its large bulbous head is upwards, and fixed to the lower end of the teſtis and epididymis, and its lower and ſlender extremity is loſt in the cellular membrane of the ſcrotum. The upper part of this ligament is within the abdomen, before the pſoas, reaching from the teſtis to the groin, or to where the teſticle is to paſs out of the abdomen; whence the ligament runs down into the ſcrotum, preciſely in the ſame manner as the ſpermatic veſſels paſs down in adult bodies, and is there loſt. The lower part of the round ligament of the uterus, in a fœtus, very much reſembles this ligament of the teſtis, and may be plainly traced down into the labium, where it is imperceptibly loſt. That part of the ligamentum teſtis, which is within the abdomen, is covered by the peritonæum all round, except at its poſterior part, which is contiguous to the pſoas, and connected with it by the reflected peritonæum, and by the cellular membrane. It is hard to ſay what is the ſtructure or compoſition of this ligament: it is certainly vaſcular and fibrous, and the fibres run in the direction of the ligament itſelf, which is covered by the fibres of the cremaſter or muſculus teſtis, placed immediately behind the peritonæum. This circumſtance is not eaſily aſcertained in the human ſubject; but is very evident in other animals, more eſpecially in thoſe whoſe teſticles remain in the cavity of the abdomen after the animal is fullgrown.

In the hedgehog, the teſtes continue through life to be lodged within the abdomen, in the ſame ſituation as in the human fœtus; and they are faſtened by the ſame kind of ligament to the inſide of the parietes of the abdomen at the groin. Now, in that animal, I find that the lowermoſt fibres of the internal oblique muſcle, which conſtitute the cremaſter, are turned inwards at the place where the ſpermatic veſſels come out in other animals, making a ſmooth edge, or lip, by their inverſion; and that then they mount up on the ligament to the lower end of the teſtis. Sometimes in the human body, and in many other animals, and very often in ſheep, the teſtes do not deſcend from the cavity of the abdomen till late in life, or never at all. In the ram, when the teſtis is come down into the ſcrotum, the cremaſter is a very ſtrong muſcle; and, though it be placed more inwards at its beginning, it paſſes down pretty much as it does in

the human body, and is loſt on the outſide of the tunica vaginalis: but in the ram, whoſe teſtis ſtill remains ſuſpended in the abdominal cavity, I find that the cremaſter ſtill exiſts, though it is a weaker muſcle; and inſtead of paſſing downwards, as in the former caſe, it turns inwards and upwards, and is loſt in the peritonæum that covers the ligament which attaches the teſtis to the parietes of the abdomen; which, in this ſtate of the animal is about an inch and a half in length. In the human fœtus, while the teſtis is retained in the cavity of the abdomen, the cremaſter is ſo ſlender that I cannot trace it to my own ſatisfaction, either turning up towards the teſtis, or turning down towards the ſcrotum. Yet from analogy we may conclude that it paſſes up to the teſticle; ſince, in the adult, we find it inſerted or loſt on the lower part of the tunica vaginalis, in the ſame manner as in the adult quadruped.

The peritonæum, which covers the teſtis and its ligament or gubernaculum, is firmly united to the ſurfaces of theſe two bodies; but all around, to wit, on the kidney, the pſoas, the iliacus internus, and the lower part of the abdominal muſcles, that membrane adheres very looſely to all the ſurfaces which it covers. Where the peritonæum is continued or reflected from the abdominal muſcles to the ligament of the teſtis, it paſſes firſt downwards a little way, as if going out of the abdomen; and then upwards, ſo as to cover more of the ligament than is within the cavity of the abdomen. At this place the peritonæum is very looſe, thin in its ſubſtance, and of a tender gelatinous texture; but all around the paſſage of that ligament, the peritonæum is conſiderably tighter, thicker, and of a more firm texture. When the abdominal muſcles are pulled up ſo as to tighten and ſtretch the peritonæum, this membrane remains looſe at the paſſage of the ligament, while it is braced or tight all around; and in that caſe the tight part forms a kind of border or edge around the looſe double part of the peritonæum, where the teſtis is afterwards to paſs. This looſe part of the peritonæum, like the intro-ſuſcepted gut, may, by drawing the teſtis upwards, be pulled up into the abdomen, and made tight; and then there is no appearance of an aperture or paſſage down towards the ſcrotum; but when the ſcrotum and ligament are drawn downwards, the looſe doubled part of the peritonæum deſcends with the

ligament, and then there is an aperture from the cavity of the abdomen all around the forepart of the ligament, which ſeems ready to receive the teſtis. This aperture becomes larger when the teſtis deſcends lower, as if the pyramidal or wedge-like ligament was firſt drawn down, in order not only to direct, but to make room for the teſtis which muſt follow it. In ſome fœtuſes I have found the aperture ſo large, that I could puſh the teſtis into it, as far as the tendon of the external oblique muſcle.

From this original ſituation within the abdomen, the teſtis afterwards deſcends to its deſtined ſtation in the ſcrotum; but it becomes difficult to aſcertain the preciſe time of this deſcent, as we hardly ever know the exact age of our ſubject. According to the obſervations which I have made, it ſeems to happen ſooner in ſome inſtances than in others; but generally about the eighth month. In the ſeventh month I have commonly found the teſtis in the abdomen; and in the ninth I have as commonly found it in the upper part of the ſcrotum. The deſcent being thus early, and the paſſage being almoſt immediately cloſed, are the principal means of preventing the hernia congenita.

At the beforementioned period, the teſtis moves downwards till its lower extremity comes into contact with the lower part of the abdominal parietes: when the upper part of the ligament, which hitherto was within the abdomen, has ſunk downwards, it lies in the paſſage from the abdomen to the ſcrotum, and in that which is afterwards to receive the teſtis. As the teſticle paſſes out, it in ſome degree inverts the ſituation of the ligament paſſing down beyond it; what was the anterior ſurface of the ligament, while in the abdomen, now becoming poſterior and compoſing the lower and anterior part of the tunica vaginalis, on which the muſculus teſtis is loſt. This is more evident in thoſe animals whoſe teſticles can readily be made to paſs up from the ſcrotum to the abdomen. The place where the ligament is moſt confined, and where the teſtis meets with moſt obſtruction in its deſcent, is the ring in the tendon of the external oblique muſcle; and accordingly I think we ſee more men with one teſtis, or both, lodged immediately within the tendon of that muſcle, than who have one or both ſtill included in the cavity of the abdomen, which I ſhall take notice of hereafter.

After the teſtis has got quite through the tendon of the external oblique muſcle, it may be conſidered as now in a way eaſily to acquire its determined ſtation; though it commonly remains for ſome time by the ſide of the penis, and only by degrees deſcends to the bottom of the ſcrotum: and when the teſtis has deſcended entirely into the ſcrotum, its ligament is ſtill connected with it, and lies immediately under it, but is ſhortened and compreſſed.

Having now given an account of the original ſituation of the teſtes, of the time of their deſcent from the abdomen, and of the route which they take in their paſſage to the ſcrotum, I ſhall in the next place deſcribe the manner in which they carry down the peritonæum with them, and then explain how that membrane forms the tunica vaginalis propria in common, and the ſac of the hernia congenita in ſome bodies.

While the teſtis is deſcending, and even when it has paſſed into the ſcrotum, it is ſtill covered by the peritonæum, exactly in the ſame manner as when within the abdomen; the ſpermatic veſſels running down behind the peritonæum there, as they did when the teſtis lay before the pſoas muſcle: that lamella of the peritonæum is united behind with the teſtis, the epididymis, and the ſpermatic veſſels, as it was in the loins, and likewiſe with the vas ferens; but the teſtis is fixed poſteriorly to the parts againſt which it reſts, being unconnected and looſe forwards, as while it remained in the abdomen. In coming down, the teſtis brings the peritonæum with it; and the elongation of that membrane; though in ſome circumſtances it be like a common hernial ſac, yet in others is very different. If we can imagine a common hernial ſac reaching to the bottom of the ſcrotum, covered by the cremaſter muſcle; and that the poſterior half of the ſac covers, and is united with, the teſtis, epididymis, ſpermatic veſſels, and vas deferens; and that the anterior half of the ſac lies looſe before all thoſe parts, it will give a perfect idea of the ſtate of the peritonæum, and of the teſtis when it comes firſt down into the ſcrotum. The teſtis, therefore, in its deſcent, does not fall looſe, like the inteſtine or epiploon, into the elongation of the peritonæum; but ſlides down from the loins, carrying the peritonæum with it; and both that and the peritonæum continue to adhere, by the cellular membrane,

to the parts behind them, as they did when in the loins. This is a circumſtance which I think may be eaſily underſtood; and yet that does not appear to be the caſe; for I find ſtudents very generally puzzled with it; imagining that, when the teſtis comes firſt down, it ſhould be looſe all round, like a piece of the gut or epiploon in a common hernia. The ductility of the peritonæum, and its very looſe connection by a ſlight cellular membrane to the pſoas muſcle, and all the other parts around the teſtis, are circumſtances which favour its elongation and deſcent into the ſcrotum with the teſtis. This peculiarity of deſcent often takes place in ſome of the inteſtines; but can only happen in thoſe which have adheſions to the loins. This I ſuſpect is only to be met with in old ruptures; never happening at the firſt formation of the hernial ſac, in which the inteſtine lies; and, I ſhould ſuppoſe, could only form very gradually. The cæcum has ſometimes been found to have deſcended into the ſcrotum, and to have brought along with it the adheſions through its whole courſe. The ſame thing has happened to the ſigmoide flexure of the colon; and I have found the whole of it in the left ſide of the ſcrotum, with its adheſions brought down from the loins. Such herniæ cannot be reduced; and in caſe of ſtrangulation, which may be brought on by a freſh portion of inteſtine coming down, are not to be treated in the common way; the ſac ſhould not be opened, but the ſtricture divided, and the newly protruded part reduced.

It is plain, from this deſcription, that the cavity of the bag, or of the elongation of the peritonæum, which contains the teſtis in the ſcrotum, muſt at firſt communicate with the general cavity of the abdomen, by an aperture at the inſide of the groin. That aperture has exactly the appearance of a common hernial ſac; the ſpermatic veſſels and vas deferens lie immediately behind it, and a probe paſſes readily through it from the general cavity of the abdomen down to the bottom of the ſcrotum. And if this proceſs of the peritonæum be laid open through its whole length on the forepart, it will be plainly ſeen to be a continuation of the peritonæum: the teſtis and epididymis will appear at the lower part of it; and the ſpermatic veſſels and the vas deferens will be found covered by the poſterior part of the bag, in their whole courſe from the groin to the teſtis.

Thus it is in the human body, when the teſtis is recently come down; and thus it is, and continues to be through life, in every quadruped which I have examined, where the teſtis is in the ſcrotum; but in the human body, the communication between the ſac and the cavity of the abdomen is ſoon cut off. Indeed I believe that the upper part of the ſac naturally begins to contract as ſoon as the teſtis has paſſed through the muſcles; which opinion is grounded on the following obſervation: In an inſtance where from the age of the fœtus, and from every other mark, it was probable that the teſtis was very recently come down, and yet the upper part of the ſac was very narrow I, puſhed the teſtis upwards, in order to ſee if it could be returned: the attachments of the teſtis eaſily admitted of its aſcent, and ſo did the aperture in the tendon of the external oblique muſcle; but the orifice and upper end of the ſac would not, by any means, admit of the teſtis being paſſed quite up into the abdomen. However this may be, the upper end of the ſac certainly contracts and unites firſt, and is quite cloſed in a very ſhort ſpace of time; for it is ſeldom that any aperture remains in a child born at its full time; and this contraction and union is continued downwards till it comes near the teſticle, where this diſpoſition does not exiſt, leaving the lower part of the ſac open or looſe, through life, even in the human ſubject, and forming the tunica teſtis vaginalis propria, the common ſeat of an hydrocele. Many caſes of hydrocele, in children, ſeem to prove that the progreſs of this contraction and union is downwards; for in them the water commonly extends higher up the chord than in the adult, except in thoſe of a conſiderable ſize: yet in ſome children this union ſeems not to take place regularly, being interrupted in the middle, and producing an hydrocele of the chord, which neither communicates with the abdomen nor tunica vaginalis teſtis. The contraction and obliteration of the paſſage appears to be a peculiar operation of Nature, depending upon ſteady and uniform principles, and not the conſequence of inflammation, nor of any thing that is accidental; and, therefore, if it is not accompliſhed at the proper time, the difficulty of bringing about an union of the parts is much greater, as is ſeen in children who have had the ſac kept open by a turn of the inteſtine falling down into the ſcrotum immediately after the teſtis.

This looks as if Nature, from being balked when ſhe was in the humour to do her work, would not, or could not ſo eaſily do it afterwards. I ſhall readily grant that what has been advanced here as a proof of the doctrine, may be explained upon other principles: but this, at leaſt, is certain, that the cloſing of the mouth, and of the neck of the ſac, is peculiar to the human ſpecies; and we muſt ſuppoſe the final intention to be, the prevention of ruptures, to which men are ſo much more liable than beaſts, from their erect ſtate of body. In ſome caſes the aperture of the ſac is not entirely cloſed, allowing a fluid to paſs down and form an hydrocele; which fluid, upon preſſure, can be ſqueezed back into the belly: and inſtances of this kind ſometimes giving the idea of a gut being protruded, make it difficult to determine the exact nature of the caſe.

What is the immediate cauſe of the deſcent of the teſtis from the loins to the ſcrotum? It is evident, that it cannot be the compreſſive force of reſpiration; becauſe, the teſtis is commonly in the ſcrotum before the child has breathed; that is, the effect has been produced before the ſuppoſed cauſe has exiſted. Is the teſtis pulled down by the cremaſter muſcle? I can hardly ſuppoſe that it is; becauſe, if that was the caſe, I ſee no reaſon why it ſhould not take place in the hedgehog, as well as in other quadrupeds; and if the muſculus teſtis had this power, it could not bring it lower than the ring of the muſcle.

Why do the teſtes take their blood-veſſels from ſuch diſtant trunks? Thoſe phyſiologiſts, who have puzzled themſelves about the ſolution of this queſtion, have not conſidered, that in the firſt formation of the body, the teſtes are ſituated, not in the ſcrotum, but immediately below the kidneys; and that, therefore, it was very natural that their blood-veſſels ſhould riſe nearly in the ſame manner as thoſe of the kidneys, but a little lower. The great length of the ſpermatic veſſels in the adult body will no doubt occaſion a more languid circulation, which, we may ſuppoſe, was the intention of Nature.

The ſituation of the teſtis in the fœtus may likewiſe account for the contrary directions of the epididymis and of the vas deferens in adult bodies, though theſe two in reality make only one excretory canal. In the fœtus the epididymis begins at the upper end of the teſtis; and it is

natural, conſidering it as an excretory tube, that it ſhould run downwards. And it is as natural that the reſt of the tube, which is called vas deferens, ſhould turn inwards at the lower end of the teſtis, becauſe that is its moſt direct courſe to the neck of the bladder. Thus we ſee that in the fœtus the excretory duct is always paſſing downwards. But the teſtis is directed in its deſcent by the gubernaculum, which is firmly fixed to the lower parts of the teſtis and epididymis, and to the beginning of the vas deferens, and thence muſt keep thoſe parts invariably in their ſituation with reſpect to one another: and therefore in proportion as the teſtis deſcends, the vas deferens muſt aſcend from the lower end of the teſtis; and it muſt, from the paſſage through the abdominal muſcles, down to the teſtis, run parallel with the ſpermatic veſſels.

The teſtis, its coats, and the ſpermatic chord, are ſo often concerned in ſome of the moſt important diſeaſes and operations of ſurgery, particularly in the bubonocele and hydrocele, that their ſtructure has been examined and deſcribed by the ſurgeons, as well as by the anatomiſts, of every age. Yet the deſcriptions of the cleareſt and beſt writers upon the ſubject differ ſo much from one another, and many of them differ ſo much from what is obvious and demonſtrable by diſſection, as to render it difficult to account for ſuch a variety of opinions. The very different ſtate of the parts in the quadruped, and in the human body, no doubt, muſt have occaſioned error and confuſion among the writers of more ancient times, when the parts of the human body were deſcribed from diſſections and obſervations made principally upon brutes: and the ſtructure of parts, which are peculiar to the fœtus, having been imperfectly underſtood, we may ſuppoſe, has likewiſe contributed to cauſe perplexity and contradiction among authors.

Baron Haller, in his Opuſcula Pathologica, has obſerved, that in infants the inteſtine ſometimes falls down into the ſcrotum after the teſtis, or along with it, and occaſions what he calls, the hernia congenita. In ſuch a caſe, the hernial ſac is formed before the inteſtine falls down, as that ingenious anatomiſt has obſerved. There are, beſides, two circumſtances peculiar to a rupture of this kind, the inteſtine being always in immediate contact

with the teſtis, and there being no tunica vaginalis propria teſtis. The ſtructure of the parts, in a fœtus, explains, in the moſt ſatisfactory manner, both theſe circumſtances, however extraordinary they muſt appear to a man who has only been accuſtomed to view the parts in ſubjects of a more advanced age; and, indeed, it is ſo clear that it needs no illuſtration. It ſhould be obſerved, however, that the hernia congenita may happen, not only by the inteſtine falling down to the teſtis before the aperture of the ſac be ſhut up, but perhaps afterwards: for when the ſac has been but recently cloſed, it ſeems poſſible enough that violence may open it again.

It muſt likewiſe be obvious to every anatomiſt, who examines the ſtate of the teſtis in children of different ages, that the mouth and neck only of the ſac cloſe up, and that the lower part of the ſac remains looſe around the teſtis, and makes the tunica vaginalis propria. Whence it is plain, that this tunic was originally a part of the elongated peritonæum; and, as it is undoubtedly the ſeat of the true hydrocele, it is alſo plain that the hernia congenita and the true hydrocele cannot exiſt together in the ſame ſide of the ſcrotum. For when there is a hernia congenita, there is no other cavity than that of the hernial ſac; and that cavity communicates with the general cavity of the abdomen.

The obſervations, contained in the two laſt paragraphs, occurred to my brother upon reading Baron Haller's Opuſcula Pathologica, and gave riſe to my inquiries upon this ſubject.

Having explained the ſituation of the teſticles in the fœtus, and their deſcent, with the circumſtances attending it, I ſhall next conſider the caſes in which the change takes place, in one or both teſticles, later than the uſual or natural time. And having remarked the conſequences of this deſcent, at ſo late a period, I ſhall take notice of thoſe inſtances in which the teſticles never paſs out of the abdomen.

I have ſaid, that the early deſcent of the teſticles, and cloſing of the mouth of the ſac, by uſually happening before birth, prevent likewiſe the deſcent of any part of the abdominal viſcera; but when the teſticles remain in their firſt ſituation beyond this period, theſe advantages are loſt; a part of the inteſtines or epiploon being, under theſe circumſtances, liable to deſcend along with them.

The firſt or natural proceſs, in ſome inſtances, not having been begun, or having been interrupted before birth, it becomes afterwards very uncertain when the deſcent will be completed: yet I think the completion moſt frequently happens between the years of two and ten, while the perſon is young and growing, being ſeldom delayed beyond the age of puberty.

It is not eaſy to aſcertain the cauſe of this failure in the deſcent of the teſticle; but I am inclined to ſuſpect that the fault originates in the teſticles themſelves: this however is certain, that the teſticle which has completed its deſcent, is the largeſt, which is more evident in the quadruped than in the human ſubject; as in theſe we can have an opportunity of examining the parts when we pleaſe, and can determine how ſmall, in compariſon with the other, that teſticle is which has exceeded the uſual time of coming down: it never deſcends ſo low as the other.

The deſcent of that teſticle is very ſlow, which is not completed before birth, often requiring years for that purpoſe; and it ſometimes never reaches the ſcrotum, eſpecially the lower part of it. There is oftner I believe an inequality in the ſituation of the two teſticles, than is commonly imagined; being ſeldom equally low in the ſcrotum: and I am of opinion that the loweſt is the moſt vigorous, having taken the lead readily, and come to its place at once. The part where it meets with the greateſt difficulty in its deſcent is, in the diviſion of the tendon of the external oblique muſcle called, the ring.

How far an erect poſition of body, the action of the abdominal muſcles, and the effect produced upon the contents of the abdomen in breathing, may contribute mechanically to the deſcent of the teſticles when the natural operations of the animal œconomy have failed, I will not pretend to decide; but when we ſee theſe combined actions producing an unnatural deſcent of a portion of inteſtine, we may conceive that they are likewiſe capable of contributing to the deſcent of the teſticle.

When the teſticle has remained in the cavity of the abdomen beyond the uſual time, it is impoſſible to ſay whether the diſpoſition for cloſing up the paſſage, after it has paſſed out, is in any degree loſt or not; but when it comes down after birth, we can eaſily ſuppoſe a portion of inteſtine or epiploon is more likely to deſcend and prevent the cloſing of

the mouth of the ſac, than before the child was born when certain actions had not taken place. We ſhould therefore watch this deſcent of the teſticle, and endeavour, by art, to procure that union which the natural powers are either not diſpoſed to perform, or are prevented from completing by the deſcent of other parts: but art ſhould not be uſed too ſoon, nor till the teſticle has got a little way below the ring. As this progreſs is very ſlow, eſpecially when the teſticle is creeping through the ring, a doubt often ariſes, whether it is better entirely to prevent its paſſage, or to aſſiſt it by exerciſe or other means; and it would certainly be the beſt practice to aſſiſt it, if that could be done effectually and ſafely. When it has got upon the outſide of the tendon it can in general be eaſily puſhed up again into the abdomen; and in theſe two ſituations it will ſometimes play backwards and forwards, for ſeveral years, without ever coming low enough to allow of the uſe of artificial means to hinder its deſcent, or to prevent a rupture. In this caſe it becomes difficult to determine what ſhould be done; but, from what I have ſeen, I ſhould be inclined to wait the deſcent, giving it every aſſiſtance in my power. Indeed, in all caſes, I would adviſe waiting with patience; for in moſt of thoſe which I have ſeen, years have elapſed from the firſt appearance of the teſticle under the ring of the abdominal muſcle before it has reached that ſituation in which we may ſafely apply a truſs. I never have perceived that any inconvenience has ariſen from waiting; and the danger, if there is any, may be in ſome degree avoided. I have always recommended moderate, not violent exerciſe.

When the teſticle has got ſome way below the ring, then the caſe is to be treated as an inguinal hernia, and a truſs applied upon the ring; taking care that the teſticle is not injured by it: but as this generally happens at too early a period for the patients themſelves to be capable of attending to it, the ſurgeon who is employed ſhould be very attentive, and thoſe in whoſe immediate care they are, particularly watchful, that no inconvenience is produced by the truſs. I have, however, known a rupture happen in a man thirty years old, where the teſticle had not even got into the ring. In ſuch a caſe, I think a truſs ſhould be immediately ap-

plied. For, if it is thought adviſable to prevent the teſticle from coming down, a truſs is equally adapted for that purpoſe, as for hindering the deſcent of an inteſtine where there is an hernial ſac.

It ſometimes happens, that one of the teſticles remains in the cavity of the abdomen through life, never acquiring the diſpoſition to change its ſituation; therefore the perſon naturally concludes that he has only one teſticle; and it can only be known that he had two by an examination of theſe parts after death: it is, however, poſſible that in ſome inſtances one may be wanting; but, if we are to reaſon from analogy, we muſt ſuppoſe this to be a very rare caſe; for it is a very common circumſtance, that many quadrupeds have only one teſticle in the ſcrotum; and in ſuch as are killed for food, and from that circumſtance come more particularly under obſervation, if this peculiarity has been noticed, we in general find the other teſticle in the cavity of the abdomen; though in ſome inſtances they are both found lying in that cavity.

When one or both teſticles remain through life in the belly, I believe that they are exceedingly imperfect, and, probably, incapable of performing their natural functions; and that this imperfection prevents the diſpoſition for deſcent from taking place. That they are more defective than even thoſe which are late in paſſing to the ſcrotum, is to be inferred from what is very evident in quadrupeds; the teſticle, that has reached the ſcrotum, being in them conſiderably larger than the one which remains in the abdomen. It is probable, that this peculiarity is a ſtep towards the hermaphrodite; the teſticle being ſeldom well formed. I have only ſeen one caſe, in the human, where both teſticles continued in the abdomen; this proved an exception to the above obſervation, ſince we are led to conclude, that they were perfectly formed, as the perſon had all the powers and paſſions of a man. In ſuch caſes nothing is to be done by art; as it is not poſſible to give the teſticles the ſtimulus of perfection, which I believe is neceſſary to make them aſſume the diſpoſition requiſite for their deſcent; and the ring of the external oblique muſcle is perhaps leſs liable, in ſuch inſtances, to allow a portion of inteſtine to puſh down, than where the teſticles have paſſed through it; and ſuch perſons may, probably, be more ſecure from accidents of this kind than if they had been more perfectly formed.

The teſticle in changing its ſituation, does not always preſerve a proper courſe towards the ſcrotum; there being inſtances of its taking another direction, and deſcending into the perinæum. How this is brought about, is difficult to ſay; it may poſſibly be occaſioned by ſomething unuſual in the conſtruction of the ſcrotum; or, more probaably, by a peculiarity in that of the perinæum itſelf. For it is not eaſy to imagine how the teſticle could make its way to the parts about the perinæum, if theſe were in a perfectly natural ſtate.

The firſt inſtance of this kind, that occurred to me, was the child of a ſhopkeeper in Oxford Street, which I viſited in company with Dr. Garthſhore, about the year 1775; but what became of the patient afterwards, I do not know. I have lately been conſulted, in a ſimilar caſe, by Mr. Hunt, a ſurgeon, at Burford in Oxfordſhire, whoſe apprehenſions of what may be the conſequences of a teſticle remaining in the perinæum, appear to be well founded. The moſt effectual method of obviating theſe will, probably, be to ſupport the teſticle in a ſituation near the groin, by the application of a bandage that may hinder its deſcent into the perinæum; by which the parts may be in time ſo conſolidated, as to retain it by the ſide of the ſcrotum.

" Dear Sir,

I take the liberty of writing to you, in conſequence of having met with a luſus naturæ of a peculiar kind, in the ſon of a man in this neighbourhood.

The boy is about twelve months old; his right teſticle is ſituated about an inch below the termination of the ſcrotum, and half an inch on the right ſide of the centre of the rapha perinæi, where a kind of pouch is formed of the common integuments, without the leaſt rugous or ſcrotal appearance on its ſurface. It is perfectly detached from the ſcrotum; nor can the teſtis or ſpermatic proceſs be at any time felt in any part of the ſcrotum, though I can readily make the teſtis paſs from its ſituation quite up into the groin; but immediately upon removing my hand, the teſtis falls down into its pouch; and I can trace the ſpermatic

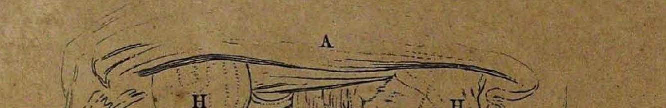
chord from the body of the teſtes up to the ring, running about a fourth of an inch on the right ſide of the ſcrotum. The ſcrotum, on each ſide, appears perfectly for ed; and the left teſtis is in ſitu naturali. Now, Sir, as I conceive this peculiar conformation may be attended with great inconvenience to the child when he comes to ride on horſe-back, and on many other occaſions, I beg leave to requeſt your opinion upon it, with reſpect to what ought to be done to prevent accidents which muſt, if left in its preſent ſituation, often occur.

Burford, (Signed) Thomas Hunt."
Oxfordſhire.

To illuſtrate the deſcriptions which I have given, I have annexed three figures that were carefully taken from Nature.

Fig. I.

Out-lines of Fig. I.



J. V. Riemsdyk del.

P. C. Canot sculp.

PLATE I.

THE firſt figure repreſents the teſtes within the abdomen, in an abortive fœtus of about ſix months. All the inteſtines, except the rectum, are removed; and the peritonæum, in moſt places is left upon the ſurfaces which it covers, ſo that the parts have not that ſharpneſs and diſtinct appearance which might have been given to them by diſſection. The outline is placed below, on which the letters of reference are marked; and the eye ſhould be carried from the deſcription to the letters of reference, and from them immediately to the correſponding part in the figure.

A The upper part of the object, covered with a cloth.
BB The thighs.
C The penis.
D The ſcrotum.
E The flap of the integuments, abdominal muſcles, and peritonæum, of the right ſide, turned back over the os ilium, to bring the teſtis into view.
F The flap of the ſkin, and cellular membrane of the left ſide, diſpoſed in the ſame manner.
G The flap of the abdominal muſcles, and of the peritonæum of the left ſide, turned back over the ſpine of the os ilium. The lower part of this flap is cut away, in order to ſhow the ligament of the teſtis paſſing down through the ring into the ſcrotum.
HH The lower part of each kidney.
I The projection formed by the lower vertebræ lumborum, and by the bifurcation of the aorta and vena cava.
K The rectum filled with meconium, and tied at its upper part where the colon was cut away.
L That branch of the inferior meſenteric artery which was going to the colon.

M The lower branch of the ſame artery, which went down into the pelvis behind the rectum.

N The poſterior ſurface of the cavity of the bladder; the anterior part, which is higher than the oſſa pubis in ſo young a fœtus, being cut away.

OO The hypogaſtric, or umbilical arteries, cut through, where they were turning up by the ſides of the bladder in their way to the navel.

PP The ureter of each ſide paſſing down before the pſoas muſcle and iliac veſſels, in its courſe to the lower part of the bladder.

QQ The ſpermatic arteries running a little ſerpentine.

RR The teſtes ſituated before the pſoæ muſcles, a little higher than the inguina. In this figure the interior edge of the teſtis is turned a little outwards, to ſhow the ſpermatic veſſels coming forwards to the poſterior edge of the teſtis, in the duplicature of the peritonæum; which duplicature connects the teſtis, incloſes its veſſels, and gives it an external ſmooth coat, much after the ſame manner as the duplicature of the meſentery connects the inteſtine, conveys it veſſels, and gives it a poliſhed covering.

The beginning of the epididymis is ſeen at the upper end of the teſtis, from which it runs down on the outſide (and therefore in this view behind the body) of the teſtis.

SS The vas deferens of each ſide, paſſing acroſs, in a ſerpentine courſe, from the extremity of the epididymis at the outſide of the lower end of the teſtis, and then before the lower part of the ureter, in its way to the veſicula ſeminalis.

TT What I have called the gubernacula, or ligaments of the teſtes in a fœtus. On the left ſide this ligament is entire, and expoſed in its whole length, the rings, ſkin of the groin, and ſcrotum, being removed, ſo that it is ſeen going down from the lower end of the teſtis into the ſcrotum; but on the right ſide, its upper and forepart is cut away, that the continuity of the epididymis and vas deferens may be ſeen; and no more of the ligament is exhibited than what is ſituated within the cavity of the abdomen.

N. B. The lower part of the ligament, as it is ſeen in the right ſide of this figure, lies ſo looſe in the paſſage through the muſcles, and is there ſo looſely covered by the peritonæum, that, when the teſtis is pulled up, more of the ligament is ſeen within the cavity of the abdomen, and then the peritonæum is made tight and ſmooth at that place; but, on the contrary, when the ſcrotum is pulled downwards, the lower part of the ligament is dragged ſome way down through the paſſage in the muſcles, and the looſe peritonæum is carried along with it; ſo that then there is a ſmall elongation of that membrane, with an orifice from the cavity of the belly, like the mouth of a ſmall hernial ſac, on the forepart of the ligament.

Out-lines of Fig. II.

Fig. II.

J. V. Riemsdyk del.

P. C. Canot sculp.

PLATE II.

THE ſecond figure repreſents nearly the ſame parts in a fœtus, ſomewhat older, in order to ſhow the ſtate of the teſtes when they have recently deſcended from the abdomen into the ſcrotum. The ſmall inteſtines are removed, and the large inteſtines are left in their natural ſituation, not now obſtructing the view of the teſtes. On the left ſide the integuments only are removed, which ſhows the chord paſſing out through the ring, with the teſticle in the vaginal coat. On the right, the ring is cut through, and the whole vaginal coat is ſlit open, expoſing the teſticle and chord.

AA The liver, in outlines.
BB The thighs, unfiniſhed.
C The penis.
D The middle part of the ſcrotum; on each ſide of which the forepart of the ſcrotum is cut away, that the teſtes may be ſeen.
EE The two flaps of the ſkin, and of the cellular membrane, diſſected off from the lower part of the abdomen, and turned down upon the thighs.
F The inteſtinum cœcum.
GG The appendicula cœci vermiformis.
H The arch of the colon.
I The turn of the colon under the ſpleen.
K The colon paſſing down on the outſide of the left kidney.
L The laſt turn of the colon, commonly called its ſigmoide flexure, which in adults is ſeated quite in the cavity of the pelvis.
M The beginning of the rectum.
N Part of the abdominal muſcles of the right ſide, with the ſmooth inveſting peritonæum, turned out over the ſpine of the os ilium.
OO The lower part of the obliquus externus muſcle of the left ſide.

P The lower part of the rectus muſcle of the right ſide, turned outwards, and towards the left ſide, ſo that the epigaſtric artery is ſeen going to the inſide of that muſcle.

Q The forepart of the bladder.

R The urachus, (as it is called.)

S The crural veſſels coming into the thigh from behind the ligamentum Fallopii.

T The external appearance of the ſpermatic rope of the left ſide.

U The external appearance of the teſtis, when its tunica vaginalis, or proceſs of the peritonæum, is a little diſtended with air or water poured into it from the cavity of the abdomen.

V The right teſtis, brought fully into view by laying open the proceſs of the peritonæum in its whole length.

W The head of the epididymis, of the ſame ſide.

XX The ſpermatic veſſels.

Y The vas deferens.

Z The ureter.

& The remains of the gubernaculum, or ligament, which bound and conducted the teſtis to the ſcrotum.

N.B. It is evident, that part of the peritonæum, which in this figure is carried down in the form of an hernial ſac to a little below the teſtis, lies before the teſtis, epididymis, ſpermatic veſſels, and vas deferens, and that it covers thoſe parts in the ſame manner as it covers the abdominal viſcera, viz. The poſterior part of the ſac (ſuppoſing the ſac to be cut lengthways into two halves) is united with them, and gives them a ſmooth ſurface; while the anterior half of the ſac lies looſe before them, and may be removed to ſome diſtance from them, as when the ſac is diſtended with water.

Fig. III

Out-lines of Fig. III

J. V. Riemsdyk del.

P. C. Canot sculp.

PLATE III.

THE third figure repreſents the teſtes, &c. in the ſame ſubject; all the parts above the oſſa iliûm being removed, and the abdominal muſcles of the left ſide turned down, to ſhow the opening of the ſac into, or from the abdomen; the bladder being likewiſe turned downwards, to ſhow the vaſa deferentia winding round behind it.

AA The thighs, unfiniſhed.

B The penis.

C The middle part of the ſcrotum, its lateral parts being removed, to ſhow the teſtes.

DD The ſkin and cellular membrane of the abdomen turned down over the thighs.

EE Part of the abdominal muſcles and peritonæum turned down at each groin.

FF The peritonæum covering the iliacus internus muſcle of each ſide.

G The inteſtinum rectum filled with meconium.

H The bladder with the umbilical artery on each ſide of it, turned a little forwards over the ſymphyſis of the pubes.

II The ureters paſſing over the iliac veſſels to the pelvis.

K The right teſtis expoſed, as in Fig. 2. V. W. XX. Y.

L The left teſtis incloſed in the proceſs of the peritonæum. See Fig. 2. U.

M The ſpermatic veſſels of the left ſide, ſeen through the peritonæum which covers them, in their deſcent through the abdominal muſcles at the groin.

N The left vas deferens ſeen through the peritonæum, in its paſſage from the mouth of the ſac to the poſterior part of the bladder.

O The mouth, or aperture of the proceſs of the peritonæum, whereby its mouth or cavity communicates with the general cavity of the

belly. This aperture clofes up, and the membrane becomes fmooth at this place, when the fœtus grows a little older; unlefs when the gut falls down after the teftis, and keeps it open: in that cafe it makes the mouth of the hernial fac.

P The left epigaftric artery branching upon the infide of the rectus muf-cle, which is here turned downwards and outwards. This artery is always fituated, as in this figure, on the infide of the mouth of the hernial fac, or paffage of the fpermatic veffels.

Plate IV.

J. V. Riemsdyk del.

W. Skelton sculp.

PLATE IV.

A ſide view of the pelvis of a young ram, to ſhow the right teſticle remaining in the cavity of the abdomen, after the left had come down, but which is removed with that half of the pelvis.

The teſticle which lies in the loins is flatter than common, and is only attached by one edge, which is principally by the epididymis; there is alſo a ligament paſſing from the upper part of the common attachment which binds the teſticle to the poſterior part of the abdominal muſcles; this is analogous to the ligament that attaches the ovarium to the ſame part in the female quadruped.

The epididymis paſſes along the outer or poſterior edge; and at the lower part becomes larger and pendulous, making a little twiſt upon itſelf where it becomes vas deferens.

The vas deferens is a little contorted, and paſſes down obliquely over the pſoas muſcle to the bladder.

From the lower end of the teſticle there is a ridge continued along the pſoas muſcle through the abdominal ring, going on to the ſcrotum, which is moſt probably the gubernaculum; but it was ſo much covered by a hard ſuety fat, that I could not exactly aſcertain its ſtructure: at the lower part of this ridge, about an inch and a half from the ring, I found the termination of the cremaſter, which was a tolerably large muſcle; part of its fibres ſeemed to ariſe in common with the internal oblique; while the reſt appeared to come from the pſoas and iliacus internus behind it; the outer portion paſſed inwards and downwards, and ſpread upon the forepart of the ridge, or gubernaculum, where the greateſt part of its fibres were loſt, and the reſt of them were continued into the backpart of it. The poſterior portion got upon the inſide of the ridge and was loſt in the ſame manner as the former.

A The inſide of the thigh, only having the outline drawn.
BB The inſide of the abdominal muſcles ſpread out.

C The ſymphyſis of the os pubis.

D The muſcles of the thigh cut through at their origin where they ariſe from a middle tendon.

E The lower end of the right kidney.

FG The iliac veſſels expoſed to ſhow their ſituation.

H The remains of the umbilical artery.

I The urinary bladder.

K The body of the right teſticle, with the ramifications of the veins upon the ſurface.

L The epididymis.

M The vas deferens.

N The veſiculæ, commonly called ſeminales.

OBSERVATIONS ON THE GLANDS SITUATED BETWEEN THE RECTUM AND BLADDER, CALLED VESICULÆ SEMINALES.

THOSE bags, in the male of ſome animals, which are ſituated between the bladder and rectum, and commonly called veſiculæ ſeminales, have been conſidered as reſervoirs of the ſemen ſecreted by the teſticles in the ſame manner as the gall-bladder is ſuppoſed to be a reſervoir of the bile. Phyſiologiſts muſt have been led to form this opinion, from obſerving, that in the human ſubject their ducts communicate with the vaſa deferentia before their termination in the urethra. This communication was ſuppoſed to allow the ſemen, when not immediately wanted, to paſs into the bags from the vaſa deferentia by a ſpecies of regurgitation. But more accurate obſervations reſpecting their ſtructure and contents in the human ſubject, and on correſponding parts in other animals ſuppoſed to anſwer a ſimilar purpoſe, joined to the circumſtance of their not being found in every claſs, induced me to conclude that this opinion was erroneous. To throw as much light upon this ſubject as poſſible, I made a number of experiments, and availed myſelf of every opportunity which offered, of examining whatever could in any way elucidate the point; and, from what I have been able to collect, I think it will appear, that they cannot be conſidered as reſervoirs of the ſemen.

To proceed regularly with my inveſtigation, I ſhall begin by comparing the contents of theſe veſiculæ with the ſemen as it is emitted from the penis of a living man; from which compariſon it will appear that, the two ſecretions are very different in their ſenſible properties of colour and ſmell; and although the ſemen which conſtitutes the firſt part of the emiſſion is evidently different from the laſt, yet every part of it is unlike the mucus found in theſe veſiculæ.

The ſemen, firſt diſcharged from the living body, is of a bluiſh white colour, in conſiſtence like cream, and ſimilar to what is found in the

vaſa deferentia after death; while that which follows, is ſomewhat like the common mucus of the noſe, but leſs viſcid. The ſemen becomes more fluid upon expoſure to the air, particularly that firſt thrown out; which is the very reverſe of what happens to ſecretions in general. The ſmell of the ſemen is maukiſh and unpleaſant, exactly reſembling that of the farina of the Spaniſh cheſnut; and, to the taſte, though at firſt inſipid, it has ſo much pungency, as, after ſome little time, to ſtimulate and excite a degree of heat in the mouth. But the fluid contained in theſe veſiculæ, in a dead body, is of a browniſh colour, and often varies in conſiſtence in different parts of the bag, as if not well mixed. Its ſmell does not reſemble that of the ſemen; neither does it become more fluid by being expoſed to the air.

It may, however, be anſwered to this, that the contents of the veſiculæ are generally found in a putrid ſtate, and have, by that means, undergone a change in their ſenſible properties. But the objection is readily obviated by comparing this fluid with that in the vaſa deferentia as it comes from the teſticles of the ſame dead body, between which there appears to be no reſemblance. To be ſtill more certain of the nature of what theſe veſiculæ contain, than was poſſible from the examination of bodies which had been dead ſome time, I took an opportunity of opening a man immediately after his death, who had been killed by a cannon-ball. The fluid in the veſiculæ was of a lighter colour than has uſually been found in men who have been dead a conſiderable time; but it was not, by any means, like the ſemen, either in colour or ſmell. In another man who died inſtantaneouſly, in conſequence of falling from a conſiderable height, whoſe body I inſpected ſoon after the accident, the contents of the veſiculæ were of a lightiſh whey colour, having nothing of the ſmell of ſemen; and in ſo fluid a ſtate as to run out on cutting into them.

I have likewiſe examined, with attention, a mucus which ſome men diſcharge upon ſtraining hard while at ſtool, or after throwing out the laſt drops of urine, an action which requires a conſiderable exertion of the parts. This diſcharge is generally called a ſeminal weakneſs, and is I believe commonly ſuppoſed to be the ſemen*; but in all the caſes of

* Vide Treatiſe on the Venereal Diſeaſe, Edit. 1ſt and 2nd. page 197.

this kind in which I have been confulted, it nearly refembled the contents of the veficulæ in the dead body, though perhaps not quite of fo deep a colour. I endeavoured, in vain, to perfuade a gentleman who had this complaint, that the difcharge was not feminal; till, by examining his own femen, and comparing it with that mucus, he was convinced of the difference. This gentleman had the power of emitting the femen in the fame quantity as ufual, immediately after the mucus had been difcharged; which is a further proof that this fluid is not femen*.

In this country, eunuchs feldom come under our examination; but we have fometimes opportunities of opening the bodies of thofe who have, in confequence of difeafe or accident, loft one or both tefticles: and feveral fubjects of this kind I have infpected after death. Perfons who have had one tefticle taken away, will better illuftrate the point in difpute, than thofe who have been deprived of both. For, it is to be prefumed, that fuch men have afterwards had connection with women, and confequently had the action of emiffion; which muft have emptied the veficulæ of the caftrated fide, if thefe had contained femen; and, as they could not be replenifhed, they fhould have been found empty after death. We have alfo, in fuch cafes, an opportunity of making comparative obfervations between the veficulæ of the perfect and thofe of the imperfect fide. In the eunuch, fuch emiffions never can happen; for the tefticles being gone, the natural and leading ftimulus is loft; therefore, if in them the veficulæ were found full after death, it might be fuppofed to be the femen which they had received from the tefticles before caftration, that had remained there from the time of the operation: but caftration being, in fuch cafes, ufually performed on children, this circumftance fhou d rather be confidered as a proof that they fecrete their own mucus. Yet it is probable, the veficulæ will neither be fo large nor fo full in eunuchs as in the perfect man; for I am of opinion, that they are connected with generation, and that if the conftitution is deprived of that power, thefe

* The difcharge was truly fuppofed to be the contents of the veficulæ; and it being imagined that thofe contained femen, according to this reafoning, the difcharge muft be feminal.

bags will not grow to the full ſize. But where only one teſticle is removed, its loſs does not in the leaſt affect generation, therefore does not produce any change in the veſicula of that ſide from which the teſticle is taken; becauſe the veſicula does not depend upon the teſticle for its ſecretion, but upon the conſtitution, and on the perſon being capable of the action of generation: therefore, as one teſticle is ſufficient to preſerve manhood, it is of courſe capable of keeping up the action of both thoſe glands.

A man who had been under my care, in St. George's Hoſpital, for a venereal complaint, died there, and was diſcovered to have loſt his right teſticle. From the cicatrix being hardly obſervable, it muſt have been removed ſome conſiderable time before his death; and the complaint for which he was received into the hoſpital is a convincing proof that he had connection with women after that period.

I inſpected the body in the preſence of Mr. Hodges, the houſe-ſurgeon, and ſeveral of the pupils of the hoſpital. Upon diſſecting out and examining the contents of the pelvis, with the penis and ſcrotum, I found that the vas deferens of the right ſide was ſmaller and firmer in its texture than the other, eſpecially at that end next to the abdominal rings, near to the part which had been cut through in the operation. The cellular membrane ſurrounding the duct, on the right ſide, was not ſo looſe as on the left; neither were the veſſels which ramified on the right veſiculæ ſo full of blood. But upon opening the veſiculæ, both appeared to be filled with the ſame kind of mucus, and ſimilar to that which is found in other dead bodies; the veſicula of the right ſide being rather larger than that on the left. Whatever, therefore, may be the real uſe of theſe veſicula, we have a proof from this diſſection, that in the human ſubject they do not contain the ſemen.

In a man, who died in St. George's Hoſpital, with a very large bubonocele, the teſticle of the diſeaſed ſide was diſcovered to have almoſt loſt its natural texture from the preſſure of the hernial ſac; and upon examining the teſticle with attention, there was no appearance of vas deferens till we came near the bladder, where it was almoſt as large as uſual.

The veſicula of that ſide was found to be as full as the other, and to contain the ſame kind of mucus.

I extirpated the left teſticle of a Frenchman, who was a married man, and died about a year afterwards, having been extremely ill for ſeveral months before his death. On examining the body, the veſiculæ were both found nearly full; more eſpecially that on the left ſide; which might be accidental: but the vas deferens of the left ſide, where it lies along this bag, and where it has a ſimilar ſtructure with the veſiculæ, was likewiſe filled with the ſame kind of mucus; which I believe is always the caſe, whether the teſticle has been removed or not.

A young man, a coachman, with his left teſticle much diſeaſed, had it removed, at St. George's Hoſpital, by Mr. Walker, in Auguſt 1785; and in February 1786, he returned again to the hoſpital, on account of uncommon pains all over him: for theſe he requeſted to be put into the warm bath; but as he was going from the ward, for that purpoſe, he dropped down and died almoſt immediately. The body was inſpected, with a view to diſcover the cauſe of his death; and, upon examination of the veſiculæ, the bag of the left ſide was as full as that on the right; and the contents in both were exactly ſimilar. In the winter 1788, another caſe occurred nearly reſembling the above.

In diſſecting a ſmale-ubject, in the year 1755, for a ſide view of the contents of the pelvis, I found a bag on the left ſide, lying contiguous to the peritonæum, juſt on the ſide of the pelvis where the internal iliac veſſels divide above the angle of reflection of the peritonæum at the union of the bladder and rectum. The left vas deferens was ſeen paſſing on to this bag; and what is very ſingular, that of the right, or oppoſite ſide, croſſed the bladder, near its union with the rectum, to join it. I traced the left vas deferens down to the teſticle; but on following the right through the ring of the external oblique muſcle, I diſcovered that it terminated at once, about an inch from its paſſage out of the abdomen, in a blunt point, which was impervious. On examining the ſpermatic chord from this point to the teſticle, I could not find any vas deferens; but by beginning at the teſticle, and tracing the epididymis from its

origin, about half way along where it lies upon the body of the teſticle, I perceived that it at firſt became ſtraight, and ſoon after ſeemed to terminate in a point. The canal at this part was ſo large, as to allow of being filled with quickſilver; which, however, did not paſs far, ſo that a portion of the epididymis was wanting; and likewiſe the vas deferens for nearly the whole length of the ſpermatic chord of the right ſide*. On the left ſide the vas deferens began where the epididymis commonly terminates; and there was a deficiency of nearly an inch of the extremity of the epididymis†. I then diſſected the bag abovementioned, which proved to be the two veſiculæ; for, by blowing air from one vas deferens, I could only inflate half of it; and from the other vas deferens, the other half. They contained the mucus commonly found in theſe bags; but upon the moſt accurate examination I could neither diſcover any duct leading from them to the proſtate gland, nor the remains of one‡.

It was evident, in this ſubject, that there was no communication between the vas deferens and epididymis; nor between theſe bags and the urethra. The caput gallinaginis had the common appearance; but there were no orifices to be found. The teſticles were very ſound, and the ducts from them to the epididymis were very manifeſt and contained ſemen**.

* Vide plate V, fig. 1. † Vide plate V, fig. 2. ‡ Vide plate VI.

** As the ſemen, in conſequence of this preternatural formation of parts, could not be conveyed to the urethra in the uſual way, I conceived it poſſible that there might be another unnatural conſtruction to make up for the deficiency in the vas deferens, and therefore examined it very carefully to ſee if there was no ſupernumerary vaſa deferentia. I was led to do this more particularly, from often finding parts reſembling them, where they could anſwer no kind of purpoſe. By a ſupernumerary vas deferens, I mean a ſmall duct which ſometimes ariſes from the epididymis, and paſſes up the ſpermatic chord along with the vas deferens, commonly terminating in a blind end, near to which it is ſometimes a little enlarged. I never found this duct go on to the urethra; but, in ſome inſtances, have ſeen it accompany the vas deferens as far as the brim of the pelvis. There is no abſolute proof that this is a ſupernumerary vas deferens; but as we find the ducts of glands in general very ſubject to ſingularities, and that there are frequently ſupernumerary ducts; that there are often two ureters to one kidney, ſometimes diſtinct from beginning to end; at other times both ariſing from one pelvis. Theſe ducts, ariſing from the epididymis, I am inclined from analogy, to believe

From theſe circumſtances we have a preſumptive proof, that the ſemen can be abſorbed in the body of the teſticle, and in the epididymis; and that the veſiculæ ſecrete a mucus which they are capable of abſorbing when it cannot be made uſe of. We may likewiſe infer from what has been ſaid, that the ſemen is not retained in reſervoirs after it is ſecreted, and kept there till it is uſed; but that it is ſecreted at the time in conſequence of certain affections of the mind ſtimulating the teſticles to this action: for we find, that if laſcivious ideas are excited in the mind, and the paroxyſm is afterwards prevented from coming on, the teſticles become painful and ſwelled from, we may ſuppoſe, the quantity of ſemen ſecreted, and the increaſed action of the veſſels; which pain and ſwelling is removed immediately upon the paroxyſm being brought on and the ſemen evacuated; but if that does not take place, the action of the veſſels will ſtill be kept up, and the pain in the teſticles in general continue till the paroxyſm and evacuation of the ſemen is brought on, to render the act complete; without which a ſtop cannot be ſo quickly put to the action of the veſſels that produce the ſecretion; nor the parts be allowed ſo eaſily to reſume their natural ſtate. There is at this time no ſenſation of any kind felt in the ſeat of the veſiculæ ſeminales; which ſhows that the action is in the teſticles, and in them alone. The pain in the teſticles, in conſequence of being filled with ſemen, and of the action being incomplete, is ſometimes ſo conſiderable as to make it neceſſary to produce an evacuation of the ſemen to relieve the patient.

It may be obſerved, in ſupport of this opinion, that theſe bags are as full of mucus in bodies much emaciated, where the perſon has died from a lingering diſeaſe, as in thoſe of the ſtrong and robuſt, whoſe death

are of a nature ſimilar to the double ureters. They reſemble the vas deferens, as being continuations of ſome of the tubes of the epididymis; are convoluted where they come off from it; afterwards become a ſtraight canal, and paſſing along with it for ſome way, they are then moſt commonly obliterated.

The idea of their being for the purpoſe of returning the ſuperfluous ſemen to the circulation, muſt certainly be erroneous, from their being ſo ſeldom met with, and ſo very ſeldom continued further than the brim of the pelvis.

has been occaſioned by violence or acute diſeaſes; and they are nearly as full in the old as in the young; which, moſt probably, would not be the caſe if they contained ſemen. Theſe facts, taken from the human ſubject, are, I think, ſufficient to eſtabliſh the opinion which I have laid down; but, for the ſatisfaction of others, I ſhall give ſuch facts and obſervations as have occurred in my diſſection of different animals, as tend to clear up the point in queſtion.

Theſe veſiculæ are not ſimilar either in ſhape or contents in any two genera of animals which I have diſſected; and they differ more in ſize, according to the bulk of the animal, than any other parts whoſe uſes in different animals are ſuppoſed to correſpond; while the ſemen in moſt of thoſe which I have examined, may be ſaid to be ſimilar.

The reſemblance which obtains between theſe bags and the gall-bladder, in the human ſubject, by no means holds equally good when applied to other animals. In the horſe they are like two ſmall urinary bladders, almoſt looſe and pendulous, with a partial coat from the peritonæum, under which there are two layers of muſcular fibres; they are thicker in their coats at the fundus than any other part; and appear there to be glandular. Their openings into the urethra are very large; and although they open cloſe to the vaſa deferentia, do not communicate with them. The ſeptum between the two ducts is not continued on quite to the urethra, ſo that they cannot, in ſtrict language, be ſaid to enter that paſſage ſeparately; but there is not length of common duct ſufficient to admit of regurgitation from the vaſa deferentia into theſe bags. They are not of the ſame ſize in the gelding and in the ſtone-horſe, being large in the laſt. Their contents in both are exactly ſimilar, and nearly equal in quantity; but in no way reſembling the ſemen emitted by the ſtone-horſe in the coitus, or what is found in the vas deferens after death.

In the boar, theſe bags are extremely large, and divided into cells of a conſiderable ſize; or they may more properly be ſaid to form ramifications cloſely connected with one another, and having a large canal or duct common to the whole. The ducts contain a whitiſh fluid, very unlike

what is found in the vaſa deferentia of the ſame animal, with which they have not the leaſt communication.

In the rat, the bags are large and flat, with ſerrated edges, and lie ſome way within the abdomen, containing a thick aſh-coloured mucus, nearly of the conſiſtence of ſoft cheeſe; very different from what is found in the vaſa deferentia of the ſame animal, with which they do not communicate.

In the beaver, the bags are convoluted; their ducts have no communication with the vaſa deferentia; but both the one and the other open on the veru montanum.

In the Guinea-pig, they are compoſed of long cylindrical tubes, and lie in the cavity of the belly; are ſmooth on their external ſurface, and do not communicate with the vaſa deferentia. They contain a thick bluiſh tranſparent ſubſtance, which is ſofteſt near the fundus, and becomes firmer towards the openings into the urethra, where it is as ſolid as common cheeſe. From this circumſtance, and what is obſerved in the horſe, the fundus appears to be the part that ſecretes this ſubſtance, which is very different in colour and conſiſtence from the contents of the vaſa deferentia, and is often found in broken pieces in the urethra.

To be more certain that the ſubſtance contained in theſe bags was not the ſecretion of the teſticle, I extracted one of the teſticles of a Guinea-pig; and ſix months afterwards gave it the female. As ſoon as the action of copulation was over, (in which all the parts containing ſemen ſhould naturally have emptied themſelves) I killed the animal, and upon examination found the veſicula of the perfect ſide, and that of the ſide from which the teſticle had been removed, both filled with a ſubſtance in every reſpect ſimilar. It will ſcarcely be alledged that this ſubſtance had been contained in the bag before the extirpation of the teſticle; nor could it be ſemen, which muſt have been all thrown out in the previous connection with the female.

To aſcertain that the contents of the veſiculæ are not diſcharged into the vagina of the female, with the ſemen, in the act of emiſſion, I killed a female Guinea-pig as ſoon as the male had left her, and examined with

attention what was contained in the vagina and uterus; in neither could I find any of the mucus of the veſiculæ; which, from its firmneſs, muſt have been eaſily detected.

In the hedge-hog, theſe bags are very large, being more than twice the ſize of the veſiculæ in the human ſubject.

Many animals have no ſuch bags; and I believe they are wanting in the greater part of that claſs which live chiefly upon animal food: they are, however, to be found in ſome of them; and the hedge-hog is an example. There is no apparent difference in the teſticles, vaſa deferentia, or ſemen of the animals which have veſiculæ and of thoſe which have none; and the mode of copulation, as far as theſe bags can be concerned, is very ſimilar in both.

In birds, as far as I have yet obſerved, there is nothing analogous to theſe bags; and yet there appears to be no difference between the mode of copulation of the drake and the bull, or ram. It is very natural to ſuppoſe, that if the veſiculæ were reſervoirs of ſemen, they would be more neceſſary in birds; who have the power of repeating the act of copulation in an infinitely greater degree than quadrupeds: and indeed we find that in birds there are reſervoirs, which may account for this power; the vaſa deferentia being enlarged juſt before they open into the rectum, probably to anſwer that intention. As birds have no urethra, ſome having merely a groove, as the drake and gander; and many being even without a groove, as the common fowl, it was abſolutely neceſſary there ſhould be ſuch a reſervoir ſomewhere; and the neceſſity of this will appear more evidently by and by.

What I have obſerved of the reſervoir of birds, is equally applicable to amphibious animals, and to that order of fiſh called rays.

From the above obſervations I think we may fairly conclude, that theſe veſiculæ are not for the purpoſe of containing ſemen; the ſingle circumſtance of their ducts being united to thoſe of the teſticles in the human ſubject not appearing ſufficient to ſet aſide the many facts which are contradictory to ſuch an opinion.

Having endeavoured to ſhow that the function of theſe veſiculæ has hitherto been miſunderſtood, the following obſervations will tend to prove that they are ſubſervient to generation, though their particular uſe is not yet diſcovered; and, for the better underſtanding this part of the ſubject, I ſhall premiſe the following facts.

Animals have their natural feelings raiſed or increaſed, according to the perfection of the parts connected with ſuch feelings. And the diſpoſition for action is alſo in proportion to the ſtate of the parts and the excitement of ſuch feelings. But that theſe feelings may be duly excited, it is neceſſary that the animal and the parts ſhould be healthy, in good condition, and in a certain degree of warmth ſuitable to that claſs to which the animal belongs. In the greateſt part of the globe there is a difference in the warmth of the ſame diſtrict at different periods, conſtituting the ſeaſons; and the cold in ſome of them is ſo conſiderable, as to prevent thoſe feelings or diſpoſitions in animals from taking place, and to render them, for the time, unfit for the purpoſes of generation*. This is owing to the teſticles becoming at this ſeaſon ſmall, and being therefore unfit to give ſuch diſpoſitions, as is the caſe in very young animals. This fact is very obvious in birds, of which the ſparrow may be produced as a proof. For if a cock-ſparrow is killed in the winter, before the days have begun to lengthen, the teſticle will be found very ſmall†; but if that organ is examined at different times in other ſparrows, as the warmth of the weather increaſes, and if this examination is continued to the breeding ſeaſon, the difference in the ſize of the teſticle will be very ſtriking‡. This circumſtance is not peculiar to birds, but is common, as far as I yet know, to all animals which have their ſeaſons of copulation. In the buck we find the teſticles are reduced to a very ſmall ſize in the winter; and in the land-mouſe, mole, &c. this diminution is ſtill more remarkable.

* It is not required that the ſeaſon for the copulation of different animals ſhould be equally warm; for the frog copulates in very cold weather; while the ſnake and lizard, which are alſo cold, ſleeping animals, do not copulate till the ſeaſon is warm.

† Vide plate VII, fig. 1.

‡ Vide plate VII, fig. 5.

Animals, on the contrary, who are not in a ſtate of Nature, have no ſuch change take place in their teſticles; and not being much affected by ſeaſons, are conſequently always in good condition, or in a ſtate to which other animals that are left to themſelves can only attain in the warmer ſeaſon. Therefore in man, who is in the ſtate we have laſt deſcribed the teſticles are nearly of the ſame ſize in winter as in ſummer: and nearly, though not exactly, the ſame thing may be obſerved in the horſe, ram, &c. theſe animals having their ſeaſons in a certain degree.

The variation above-taken notice of, is not confined to the teſticles; but alſo extends to the parts which are connected with them. For, in thoſe animals that have their ſeaſons for propagation, the moſt diſtinctly marked, as the land-mouſe, mole, &c. the veſiculæ are hardly diſcernable in the winter; but in the ſpring are very large, varying in ſize in a manner ſimilar to the teſticle. It may, however, be alledged, that the change in theſe bags might naturally be ſuppoſed to take place, even admitting them to be ſeminal reſervoirs: but what happens to the proſtate gland, which has never been ſuppoſed to contain ſemen, will take off the force of this objection; ſince in all the animals which have ſuch a gland (and which have their ſeaſon for propagation) it undergoes a ſimilar change. In the mole, the proſtate gland in winter is hardly diſcernable, but in the ſpring becomes very large and is filled with mucus.

From theſe obſervations it is reaſonable to infer, that the uſe of the veſiculæ in the animal œconomy muſt, in common with many other parts, be dependent upon the teſticles. For the penis, urethra, and all the parts connected with them, are ſo far ſubſervient to the teſticles, that I am perſuaded few of them would have exiſted if there had been no teſticles in the original conſtruction of the body; and theſe would have been ſo formed as merely to aſſiſt in the expulſion of the urine. To illuſtrate this opinion, let us obſerve what is the difference between theſe parts in the perfect male, and in a male that has been deprived of the teſticles when very young, at an age in which they have had no ſuch influence upon the animal œconomy as to affect the growth of the other

parts. In the perfect male the penis is large; the corpora cavernosa* being capable of dilatation. The corpus spongiosum is very vascular†; that part of the canal which is called the bulb is considerably enlarged, forming a cavity; and the musculi acceleratores urinæ, as they are termed, are strong and healthy. In many animals which have a long penis, the muscular fibres are continued forwards to the end of it; and in others, though not extended so far, they are very large.

On the contrary, in the castrated animal the penis is small, and not capable of much dilatation; the corpus spongiosum is less vascular; the cavity at the bulb is little larger than the canal of the urethra; and the muscles are white, small, and have a ligamentous appearance. The same observations are true, if applied to the erectores penis.

The penis of the perfect male is of a sufficient length, when erected, to reach to the further end of the vagina of the female. In the castrated animal it is much shorter; and erections having then become unnecessary, the parts which should project, often adhere to the inside of the prepuce. The erectores muscles in the perfect male are strong enough to squeeze at once the blood out of the crura into the body of the penis, so as to straiten and contract the urethra instantaneously, and the acceleratores urinæ‡ have sufficient power to throw out the semen that is gradually accumulated at the bulb for ejection.

* The cells of the corpora cavernosa are muscular, although no such appearance is to be observed in men: for the penis in erection is not at all times equally distended. The penis in a cold day is not so large in erection as in a warm one; which, probably, arises from a kind of spasm that could not act upon it if it were not muscular.

In the horse, the parts composing the cells of the penis appear evidently muscular to the eye; and, in a horse just killed, they contract upon being stimulated.

† It may not be improper to observe, that the corpus spongiosum urethæ, and glans penis, are not spongy or cellular, but made up of a plexus of veins. This structure is discernable in the human subject; but much more distinctly seen in many animals, as the horse, &c.

‡ I shall call these muscles, expulsores seminis, as I apprehend their real use to be for the expulsion of that secretion: these muscles likewise throw out those drops of urine which are collected in the bulb from the last contractions of the bladder; and they have been, from this circumstance, named acceleratores urinæ; but if a receptacle had not been necessary for the semen, those muscles had probably never existed; and the last drops of urine would have been

The proſtate gland*, Cowper's glands, and the glands along the urethra, (of which the lacunæ are the excretory ducts) are in the perfect male large and pulpy, ſecreting a conſiderable quantity of ſlimy mucus, which is ſalt to the taſte; it is moſt probably for the purpoſe of lubricating thoſe parts, and is only thrown out when in vigour for copulation: while in the caſtrated animal theſe are ſmall, flabby, tough and ligamentous, and have little ſecretion. From this account there appears to be an eſſential difference between the parts connected with generation of the perfect male, and thoſe which remain in one that has been caſtrated; more eſpecially if that operation had been performed while the animal was young.

If it is objected, that the ſame changes did not take place in the men from whom one teſticle had been removed; it may be anſwered, that the operation was performed late in life: and one teſticle being left, that was ſufficient to carry on the neceſſary actions, and conſequently to preſerve the powers; therefore whatever parts had a connection with theſe powers would ſtill have the ſtimulus of perfection given to them.

The different appearance of the bulb, and the muſcles, would ſeem to point out, in the perfect male, the enlargement of the bulb to be for the purpoſe of a receptacle for the ſemen; for although I have denied the veſiculæ to be reſervoirs, yet as it was neceſſary that the ſemen ſhould be accumulated ſomewhere before ejection, I ſhall endeavour to prove, from the mode of copulation in the animals we are beſt acquainted with,

thrown out by the action of the bladder and urethra, as in ſome meaſure is the caſe in the caſtrated animal. That the urethra has the power of contraction, is evident upon the application of any ſtimulus: for I have ſeen the urethra refuſe to allow an injection to paſs on: and in that part where the injection ſtopped, a fulneſs was felt which terminated at once: this contraction is moſt probably in the internal membrane; it alſo will often refuſe the paſſage of a bougie.

* The proſtate gland is not common to all animals. It is wanting in the bull, buck, and moſt probably, I believe, in all ruminating animals. In this claſs the coats of the veſiculæ are much thicker, and more glandular, than in thoſe who have proſtate glands; it is therefore natural to ſuppoſe, that the veſiculæ anſwer nearly the ſame purpoſes as the proſtate gland.

The proſtate gland, and Cowper's glands, as well as the veſiculæ, are wanting in birds, in the amphibious animals, and in thoſe fiſh which have teſticles, as all of the ray kind.

that the bulb is intended for that purpoſe. Let us, therefore, give a ſhort account of the different parts concerned in coition; and by obſerving the dependence which they have upon one another, ſee how this proof will come out.

The erection of the penis is produced by a ſtop being put to the returning blood; and this ſtoppage is ſo complete, that no mechanical preſſure applied to the body of the penis, can force the blood on into the veins. This erection anſwers two purpoſes; it gives ſize and ſtrength to the penis, and it renders the canal of the urethra ſmaller. The corpus ſpongioſum of the urethra, and the glans, which is only a continuation of it, are filled with blood from the ſame cauſe, but not ſo completely as the body of the penis; ſince from them it can be forced out into the veins by preſſure*. This accumulation of blood in the corpus ſpongioſum diminiſhes the canal of the urethra ſo much, that any preſſure upon one part of it will have a conſiderable effect upon the other; not only by leſſening its capacity at the part preſſed; but by forcing the blood forward, the parts beyond will be ſtill more diſtended, and conſequently the canal of the urethra be in that proportion diminiſhed. The ſemen, in the time of copulation, in ſuch animals as remain long in that act, is gradually ſqueezed along the vaſa deferentia (as it is ſecreted) into the bulb; and when the teſticles ceaſe to ſecrete, the paroxyſm, which is to finiſh the whole operation, comes on. The ſemen acting as a ſtimulus to the cavity of the bulb of the urethra, the muſcles of that part of the canal are thrown into action; the fibres neareſt the bladder, probably, act firſt, and thoſe more forward in quick ſucceſſion; the ſemen is projected with ſome force; the blood in the bulb of the urethra is by the ſame

* In April 1760, in the preſence of Mr. Blount, I laid bare the penis of a dog, almoſt through its whole length; traced the two veins that came from the glans, (which, in this animal makes the largeſt part of the penis) and ſeparated them from the arteries by diſſection, that I might be able to compreſs them at pleaſure without affecting the arteries. I then compreſſed the two veins, and found that the glans and large bulb became full and extended; but when I irritated the veins, in order to ſee if there was any power of contraction in them which might occaſionally ſtop the return of the blood, no ſuch appearance could be obſerved.

action squeezed forward; but requiring a greater impulse to propel it, is rather later than the semen, on which it presses from behind; the corpus spongiosum being full of blood, acts almost as quick as undulation, in which it is assisted by the corresponding constriction of the urethra, and the semen is hurried along with a considerable velocity.

From the facts which I have stated respecting the organs of generation, the observations which I have made, and the series of actions which I have considered as taking place in the copulation of animals, I think the following inferences may be fairly drawn.

That the bags, called vesiculæ seminales, are not seminal reservoirs, but glands secreting a peculiar mucus; and that the bulb of the uretha, is, properly speaking, the receptacle in which the semen is accumulated previous to ejection.

Although it seems to have been proved that the vesiculæ do not contain the semen, I have not been able to ascertain their particular use; we may, however, be allowed upon the whole to conclude that they are, together with other parts, subservient to the purposes of generation.

Plate V.

Fig. I.

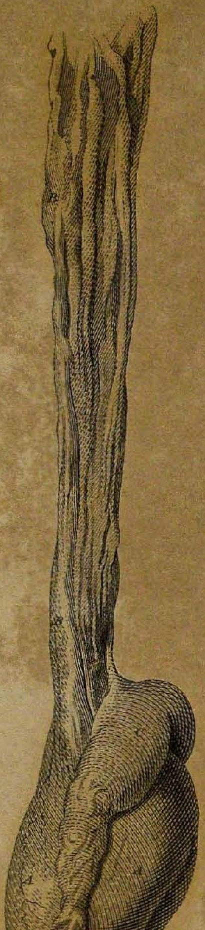


Fig. II.

J. V. Riemsdyk del.

Wm. Skelton sculp.

PLATE V.

SHOWS two teſticles, with the ſpermatic chords diſſected; in the one the vas deferens, in the other a portion of the epididymis, is wanting.

Figure 1. The right teſticle and ſpermatic chord.
AA The body of the teſticle.
BB The ſpermatic chord, in which there is no appearance of vas deferens.
C The epididymis, where it takes its origin from the body of the teſticle.
D The abrupt termination of the epididymis, it not being continued to the lower end of the teſticle.

Figure 2. The left teſticle.
AA The body of the teſticle.
B The blood-veſſels of the teſticle ſeparated from the vas deferens.
C The origin of the epididymis.
D The termination of the epididymis; to ſhow which, the tunica vaginalis is removed.
E The origin of the vas deferens.
F The vas deferens, as it paſſes up towards the ring of the abdominal muſcles.

Plate VI.

J. V. Riemsdyk del.

Wm. Skelton sculp.

PLATE VI.

A ſide view of the pelvis, taken from the ſame ſubject as plate V, in which the vaſa deferentia did not communicate with the veſiculæ, and the veſiculæ did not communicate with the urethra.

A The body of the penis.
B The ſymphyſis of the pubis.
C The bladder.
D The left ureter.
E E The rectum.
F The anus.
G The ſphincter muſcle of the anus, turned aſide.
H The levator muſcle of the anus, turned down.
I The proſtate gland.
K The Cowper's gland of the left ſide.
L The peritonæum, which lined the left ſide of the pelvis.
M The ſacrum, where it is articulated with the os ilium.
N The left vas deferens.
O The veſiculæ.

Plate VII

1 January

2 Middle of February

3 Beginning of March

4 Latterend of March

5 Middle of April

PLATE VII.

TO ſhow the gradual increaſe, in ſize, of the teſtes of the ſparrow, from the middle of winter to the beginning of the breeding ſeaſon, I examined thoſe glands in January, February, March and April; and the appearances they put on at theſe different periods are faithfully repreſented in the plate, with the date of their examination annexed to each.

If we compare their ſize in January, with what it is in April, it hardly appears poſſible that ſuch a wonderful change could have taken place during ſo ſhort a period.

ELEVEN of the following papers have been read at the Royal Society, and publiſhed in the Philoſophical Tranſactions; but in a work of ſo general a nature, and of which phyſiological inquiries make ſo ſmall a part, the few facts and obſervations which I have given upon ſuch ſubjects may, probably, be overlooked by thoſe who are not members of that ſociety. That they may be more eaſily procured by ſtudents in medicine, and other readers, I have, by an application to the Preſident and Council of the Royal Society, obtained leave to reprint them, in this work, as being connected with the principles and actions of the animal œconomy; and I have added ſuch obſervations and remarks as have occurred to me ſince the time they were read before the Royal Society.

ACCOUNT OF THE FREE MARTIN.

GENERATION, from a ſeed, requires the concurrence of two cauſes to give it perfection; the one to form the ſeed, the other to give it the principle of action*.

The cauſe, forming the ſeed, is called the female; the other, the male: but thoſe two cauſes, in general, make only a part of a whole animal, or are rather parts ſuperadded to an animal. Probably theſe characteriſtics were firſt obſerved in ſuch animals as had the female parts complete in one, and the male in the other; therefore the terms, female and male, have been applied to the whole animal, dividing them into two diſtinct ſexes; and the parts, which formed the one ſex or the other, were called the female, or the male parts of generation; but, upon a more accurate knowledge of animals, and of their parts of generation, theſe were found in many of the inferior tribes to be united in the ſame animal, which, from poſſeſſing both, has got the name of hermaphrodite.

As the diſtinction of male or female parts is natural to moſt animals, as the union of them in the ſame animal is alſo natural to many, and as the ſeparation of them is only a circumſtance making no eſſential dif-

* It may be neceſſary for ſome of my readers to have explained to them what I mean by a ſeed. I do ſuppoſe that, the word ſeed was firſt applied to grain, or that which is always called ſeed in the vegetable; which ſeed is the part of ſuch vegetables in which the matter of the young vegetable exiſts, or is formed. The principle of arrangement in the farina, or male part, fitting the ſeed for action, being at firſt not known, a falſe analogy between the vegetable and animal was eſtabliſhed, and the matter ſecreted by the teſtes was called the ſeed: but, from the knowledge of the diſtinct ſexes in the vegetable, it is well known that, the ſeed is the female production in them, and that the principle of arrangement for action is from the male. The ſame operation and principles take place in many orders of animals, the female producing a ſeed in which is the matter fitted for the firſt arrangement of the organs of the animal, and which receives the principle of arrangement fitting them for action from the male.

ference in the ſtructure of the parts themſelves, it becomes no great effort or uncommon play in Nature, ſometimes to unite them in thoſe animals in which they are commonly ſeparated; a circumſtance we really find takes place in many animals of thoſe order in which ſuch an union is unnatural. From this ſtate of the caſe, hermaphrodites may be divided into two kinds, the natural and unnatural.

The natural hermaphrodite belongs to the inferior and more ſimple genera of animals, of which there is a much greater number than of the more perfect; and as animals become more complicated, have more parts, and each part is more confined to its particular uſe, a ſeparation of the two neceſſary powers for generation ſeems alſo to take place.

The unnatural hermaphrodite, I believe, now and then occurs in every tribe of animals having diſtinct ſexes; but is more common in ſome than in others*; and is to be met with, in all its gradations, from the diſtinct ſex to the moſt exact combination of male and female organs. This, I fancy, happens moſt rarely in the human ſpecies, never having ſeen an inſtance. I can ſay the ſame of dogs and cats, with which laſt, however, I am leſs acquainted; but in the horſe, aſs, ſheep, and black cattle, it is very frequent.

There is one part common to both the male and female organs of generation in all animals which have the ſexes diſtinct; in the one ſex it is called the penis; in the other, the clitoris; its ſpecific uſe, in both, is to continue, by its ſenſibility, the action excited in coition till the paroxyſm alters the ſenſation. In the female it probably anſwers no other purpoſe; but in the male it is more complicated, to adapt it for the purpoſe of conducting and expelling the ſemen that has been ſecreted in conſequence of the actions ſo excited.

Though the unnatural hermaphrodite be a mixture of both ſexes, and may poſſeſs the parts peculiar to each in perfection, yet it cannot poſſeſs in perfection that part which is common to both. For as this common

* Quere: Is there ever, in the genera of animals that are natural hermaphrodites, a ſeparation of the two parts forming diſtinct ſexes? If there is, it may account for the diſtinction of ſexes ever having happened.

part is different in one ſex from what it is in the other, and it is impoſſible for one animal to have both a penis and clitoris, the common part muſt of courſe partake of both ſexes, and conſequently render the harmaphrodite ſo far incomplete; but theſe parts, peculiar to each ſex, may be perfectly joined in the ſame animal, which will convey an idea of the trueſt hermaphrodite. Although it may not be neceſſary to conſtitute an hermaphrodite, that the parts peculiar to the one ſex ſhould be blended with thoſe of the other, in the ſame way that the penis is with the clitoris; yet this ſometimes takes place in parts whoſe uſe in the diſtinct ſexes is ſomewhat ſimilar, the teſticle and ovarium ſometimes forming one body, without the properties of either. This compounded part in thoſe animals that have the teſticle and ovarium differently ſituated, is generally found in the place allotted for the ovarium; but in ſuch animals as have the teſticle and ovarium in the ſame ſituation as the bird tribe, the compound of the two, when it occurs, will alſo be found in that common ſituation.

" The parts of the female, appropriated for the purpoſe of ſupplying the young with nouriſhment, are variouſly placed in different animals. In the horſe, black cattle, ſheep, and other granivorous animals, their ſituation is between the hind legs; and this being alſo the place allotted for the teſticles of the male of this tribe, and probably of all thoſe in which the teſticles come out of the cavity of the belly; in the hermaphrodite, therefore, which has both theſe parts, the teſticles muſt, to a certain degree, deſcend into the udder, though that cannot receive them ſo readily as the ſcrotum.

The hermaphrodites, which I have ſeen, have always appeared externally, and, at firſt view, to be females, from the penis being the part principally deficient, and there being an opening behind like the bearing in the female; and as the teſticles in ſuch hermaphrodites ſeldom come down, the udder is left to occupy its proper place. In animals the female of which is preſerved for breeding only, as ſheep, goats, pigs, &c. theſe are generally kept, from their being ſuppoſed to be females.

Among horſes ſuch hermaphrodites are very frequent: I have ſeen ſeveral, but never diſſected any. The moſt complete was one in which

the teſticles had come down out of the abdomen into the place where the udder ſhould have been, (viz. more forward than the ſcrotum) and though not ſo pendulous as the ſcrotum in the perfect male of ſuch animals, had all the appearance of an udder. There were alſo two diſtinct nipples, which, although they exiſt in the male, have no perfect form, being blended with the ſheath or prepuce, of which there was none here. The external female parts were exactly ſimilar to thoſe of the perfect female; but inſtead of a common ſized clitoris, there was one about five or ſix inches long, which, when erect, pointed almoſt directly backwards.

I procured a foal-aſs, very ſimilar in external appearance to the horſe, abovementioned, and killed it to examine the parts. It had two nipples, but the teſticles were not come down as in the above; owing, perhaps, to the animal's being yet too young.

There was no penis paſſing round the pubis to the belly, as in the perfect male aſs.

The external female parts were ſimilar to thoſe of the ſhe-aſs. Within the entrance of the vagina was placed the clitoris; but much longer than that of a true female, it meaſuring about five inches. The vagina was pervious a little beyond the opening of the urethra into it, and from thence up to the fundus of the uterus there was no canal. The uterus was hollow at the fundus, or had a cavity in it, and then divided into two horns, which were alſo pervious. Beyond the termination of the two horns were placed the ovaria as in the true female; but I could not find the Fallopian tubes. From the broad ligaments, to the edges of which the horns of the uterus and ovaria are attached, there paſſed towards each groin a part ſimilar to the round ligaments in the female, which were continued into the rings of the abdominal muſcles; but with this difference, that there accompanied them a proceſs or theca of the peritonæum, ſimilar to the tunica vaginalis communis in the male aſs; and in theſe thecæ were found the teſticles; but I could not obſerve any vaſa deferentia paſſing from them.

Here then were found, in the ſame animal, the parts peculiar to each ſex, (although very imperfect) and that part which is common to both, but different in each, was a kind of medium of that difference.

Something ſimilar to the above I have ſeen in ſheep, goats, &c. but I ſhall not at preſent trouble the reader with a deſcription of hermaphrodites in general, as it is a very extenſive ſubject, admitting of great variety, which would make them appear a production of chance; whereas the intention of this account is to point out a circumſtance which takes place in the production of hermaphrodites in black cattle, that appears to be almoſt an eſtabliſhed principle in their propagation; and is, perhaps, peculiar to that ſpecies of animals.

It is a fact known, and I believe almoſt univerſally underſtood, that when a cow brings forth two calves, and one of them a bull-calf, and the other to appearance a cow, that the cow-calf is unfit for propagation; but the bull-calf grows up into a very proper bull. Such a cow-calf is called, in this country, a FREE MARTIN; and is commonly as well known among the farmers as either cow or bull. Although it will appear, from the deſcription of this animal, that it is an hermaphrodite, (being in no reſpect different from other hermaphrodites) yet I ſhall retain the term, free martin, to diſtinguiſh the hermaphrodite produced in this way, from thoſe which reſemble the hermaphrodite of other animals: for I know that in black cattle, ſuch a deviation may be produced without the circumſtance of twins; and even when there are twins, the one a male the other a female, they may both have the organs of generation perfectly formed. But when I ſpeak of thoſe which are not twins, I ſhall call them hermaphrodites; the only circumſtance worth our notice being a ſingularity in the mode of production of the free martin, and its being, as far as I yet know, peculiar to black cattle.

This calf has all the external marks of a cow-calf, ſimilar to what was mentioned in the unnatural hermaphrodite, viz. the teats and the external female parts, called by farmers, the bearing; and when they are preſerved, by thoſe who know the above fact, it is not for propagation, but for all the purpoſes of an ox or ſpayed heifer, viz. to yoke with the oxen, and to fatten for the table*.

* I need hardly obſerve here, that if a cow has twins, and they are both bull-calves, that they are in every reſpect perfect bulls; or if they are both cow-calves, they are perfect cows.

It is known, that they do not breed; they do not ſhow the leaſt inclination for the bull; nor does the bull ever take the leaſt notice of them*. They very much reſemble, in form, the ox or ſpayed heifer, being conſiderably larger than either the bull or the cow, having the horns very ſimilar to the horns of an ox.

The bellow of the free martin is ſimilar to that of an ox, having more reſemblance to that of the cow than of the bull. Free martins are very much diſpoſed to grow fat with good food. The fleſh, like that of the ox or ſpayed heifer, is generally much finer in the fibre than either the bull or cow; is ſuppoſed to exceed that of the ox and heifer in delicacy of flavour; and bears a higher price at market.

However, it ſeems that this is not univerſal; for I was lately informed by Charles Palmer, Eſq. of Luckley in Berkſhire, that a free martin having been killed in his neighbourhood, from the general idea of its being better meat than common, every neighbour beſpoke a piece, which turned out nearly as bad as bull-beef; worſe at leaſt than that of a cow. It is probable that circumſtance might ariſe from this animal having more the properties of a bull than the cow; as we ſhall ſee hereafter, that they are ſometimes more the one than the other†.

Although what I have advanced with reſpect to the production of free martins be in general true, yet by the aſſiſtance of Benjamin Way, Eſq. of Denham, near Uxbridge, who knew my anxiety to aſcertain this point, I was lately furniſhed with an inſtance which proves that it does not invariably hold good.

One of his cows having produced twins, which were to appearance male and female; upon a ſuppoſition that the cow-calf was a free martin,

* Vide Leſlie on Huſbandry, p. 98, 99.

† The Romans called the bull, taurus; they, however, talked of tauræ in the feminine gender. And Stephen obſerves, that it was thought the Romans meant by tauræ, barren cows, and called them by this name becauſe they did not conceive. He alſo quotes a paſſage from Columella, lib. vi. cap. 22. "and like the tauræ, which occupy the place of fertile cows, ſhould be rejected, or ſent away." He likewiſe quotes Varro, De re Ruſtica, lib. ii. cap. 5. "The cow which is barren, is called taura." From which we may reaſonably conjecture that the Romans had not the idea of the circumſtances of their production.

he obligingly offered either to give it me, or to keep it till it grew up, that we might determine the fact: as I conceived it to be a free martin, and was to have the liberty of examining it after death, I desired that he would keep it; but, unfortunately, it died at about a month old. Upon examining the organs of generation, they appeared to be those of the female, and perfectly formed; but to make this more certain, I procured those of a common cow-calf, and comparing them together, found them exactly alike. This made us regret that the animal had not lived to an age that might have determined if it was capable of breeding; for the construction of the parts being to appearance perfect, is not sufficient of itself to stamp it a true or perfect female; as I can suppose that, the parts being perfectly formed, but without the power of propagation, may constitute the most simple kind of hermaphrodite. It is, however, most probable, that this was a perfect female, which is an exception to the common rule; and I have been informed, there are instances of such twins breeding. If there are such deviations, as of twins being perfect male and female, why should there not be, on the other hand, an hermaphrodite produced singly, as in other animals? I had the examination of one which seemed, upon the strictest inquiry, to have been a single calf; and I am the more inclined to think this true, from having found a number of hermaphrodites among black cattle, without the circumstances of their birth being ascertained.

Hermaphrodites are to be met with in sheep; but, from the account given of them, I should suppose that they are not free martins. I have seen several which were supposed to be hermaphrodites, but which were imperfect males, having the penis terminating in the perinæum; the orifice of which appeared like the bearing in the female. Such are not naturally stimulated to put themselves in the position of the female when they void their urine, so that when it passes, the surrounding parts are wetted by it, and being covered with wool, and retaining the urine, keeps them continually moist, and gives the animal a strong smell. They are mentioned as both male and female.

I believe it had never been even conjectured, notwithstanding all these peculiarities, what was the true nature of the free martin; and from

the ſingularity of the animal, and the account of its production, I was almoſt tempted to ſuppoſe the whole a vulgar error. Yet by the univerſality of the teſtimony in its favour, it appearing to have ſome foundation, I eagerly ſought for an opportunity to ſee and examine them. I have ſucceeded in this inquiry, and have ſeen ſeveral; the firſt of which was one belonging to John Arbuthnot, Eſq. of Mitcham, and was calved in his own farm. He was ſo obliging as to allow me to ſatisfy myſelf, firſt by permitting a drawing to be made of the animal while alive, which was executed by Mr. Gilpin; and afterwards to examine the parts when the animal died. At the time the drawing was made of Mr. Arbuthnot's free martin, John Wells, Eſq. of Bickley Farm, near Bromley in Kent, was preſent, and informed us, that a cow of his had calved two calves, one of which was a bull-calf and the other a cow-calf. I deſired Mr. Arbuthnot to requeſt Mr. Wells to keep them, or let me buy them of him; but, from his great deſire of natural knowledge, he very readily conſented to preſerve both till the bull ſhewed all the ſigns of a good bull; and when the free martin was killed he allowed me to inſpect the parts.

Of all the ſpecimens which I have diſſected, I ſhall only give the deſcriptions of the three which point out moſt diſtinctly the complete free martin, with the gradations towards the male and female.

THE DESCRIPTION OF THE THREE FREE MARTINS.

MR. WRIGHT'S FREE MARTIN, FIVE YEARS OLD.

This animal had more the appearance and general character of the ox, or ſpayed heifer, than of either the bull or cow. The vagina terminated in a blind end, a little way beyond the opening of the urethra, from which the vagina and uterus were impervious. The uterus, at its extreme part, divided into two horns. At the termination of the horns were placed the teſticles inſtead of the ovaria, as is the caſe in the female. The reaſons why I call theſe bodies, teſticles, are the following. Firſt,

they were above twenty times larger than the ovaria of the cow, and nearly the ſize of the teſticles of the bull; or rather of thoſe of the ridgil, the bull whoſe teſticles never come down. Secondly, the ſpermatic arteries were ſimilar to thoſe of the bull, eſpecially of the ridgil. Thirdly, the cremaſter muſcle paſſed up from the rings of the abdominal muſcles to the teſticles, as it does in the ridgil*.

There were the two bags placed behind, between the bladder and the uterus. Their ducts opened into the vagina, a very little way beyond the opening of the urethra; but there was nothing ſimilar to the vaſa deferentia.

As the external parts had more of the cow than the bull, the clitoris, which may be reckoned an external part, was alſo ſimilar to that of the cow; not at all in a middle ſtate between the penis of the bull and the clitoris of the cow, as I have deſcribed in the hermaphrodite horſe. There were four teats; the glandular part of the udder was but ſmall.

This animal cannot be ſaid to have been a mixture of all the parts of both ſexes, for the clitoris had nothing ſimilar to the penis in the male, and it was deficient in the female parts, by having nothing ſimilar to the ovaria; neither had the uterus a cavity.

MR. ARBUTHNOT'S FREE MARTIN†.

The external parts were rather ſmaller than in the cow. The vagina paſſed on, as in the cow, to the opening of the urethra, and then it began to contract into a ſmall canal, which paſſed on to the diviſion of

* Although I call theſe bodies teſticles, for the reaſon given, yet they were only imitations of them; for when cut into, they had nothing of the ſtructure of the teſticle: not being ſimilar to any thing in Nature, they had more the appearance of diſeaſe. From the ſeeming imperfection of the animal itſelf, it was not to be ſuppoſed that they ſhould be teſticles, for then the animal ſhould have partaken of the bull, which it certainly did not.

† This animal was ſeven years old; had been often yoked with the oxen; at other times went with the cows and bull; but never ſhewed any deſires for either the one or the other.

the uterus into the two horns, each horn paſſing along the edge of the broad ligament laterally towards the ovaria.

At the termination of theſe horns were placed both the ovaria and the teſticles; they were nearly of the ſame ſize, and about as large as a ſmall nutmeg.

To the ovaria I could not find any Fallopian tube.

To the teſticles were vaſa deferentia, but they were imperfect. The left one did not reach near to the teſticle; the right only came cloſe to it, but did not terminate in a body, called the epididymis. They were both pervious, and opened into the vagina near the opening of the urethra.

On the poſterior ſurface of the bladder, or between the uterus and bladder, were the two bags, called veſiculæ ſeminales in the male, but much ſmaller than what they are in the bull; the ducts opened along with the vaſa deferentia. This was more entitled to the name of hermaphrodite than the firſt or third; for it had a mixture of all the parts, though all were imperfect.

MR. WELLS'S FREE MARTIN.

This animal was between three and four years old when killed; and had never been obſerved to ſhow any ſigns of deſire for the male, although it went conſtantly with one; and looked more like an heifer than free martins uſually do.

The teats and udder were ſmall compared with thoſe of an heifer; but rather larger than in either of the former examples; the beginning of the vagina was ſimilar to that of the cow, but ſoon terminated a little beyond the opening of the urethra, as in the firſt deſcribed. The vagina and uterus, to external appearance, were continued, although not pervious; and the uterine part divided into two horns, at the end of which were the ovaria.

I could not obſerve in this animal any other body which I could ſuppoſe to be the teſticle.

There was on the ſide of the uterus an interrupted vas deferens broken off in ſeveral places.

Behind the bladder, or between that and the vagina, were the bags called veſiculæ ſeminales; between which were the terminations of the two vaſa deferentia.

The ducts of the bags, and the vaſa deferentia, opened as in the laſt inſtance.

This could not be called an exact mixture of all the parts of both ſexes, for here was no appearance of teſticles.

The female parts were imperfect, and there was the addition of part of the vaſa deferentia, and the bags called veſiculæ ſeminales.

This circumſtance of having no teſticles, perhaps, was the reaſon why it had more the external appearance of an heifer than what they commonly have, and more than either of the two former.

PLATE VIII.

THIS plate is a reprefentation of Mr. Wright's free martin, taken from a drawing of the living animal, by Mr. Gilpin. It fhows the external form of that animal, which is neither like the bull nor cow; but refembling the ox or fpayed heifer.

Plate IX

Wm. Bell del.

Wm. Skelton sculp.

PLATE IX.

THIS plate repreſents the organs of generation of Mr. Wright's free martin, which are more the parts of a bull than thoſe of a cow; and the animal, while alive, had a good deal the character and look of an ox.

A The peak of the labia.
BB The labia.
C The glans clitoridis.
DDDD The inner ſurface of the common vagina.
EE The orifices of the ducts of two glands.
F Meatus urinarius.
GG The inner ſurface of the true vagina, terminating in a blind end at H.
H The termination of the vagina in a blind end.
IIII What may be called uterus, but impervious.
KK What may be called horns of the uterus.
LL The teſticles.
MM The ſpermatic veſſels.
NN The cremaſter muſcles.
OO The veſiculæ ſeminales.
PP The ducts of the veſiculæ ſeminales ſeen through the vagina.
Q Points to the ducts of ditto, into which are introduced briſtles.

Plate X.

Wm. Bell del.

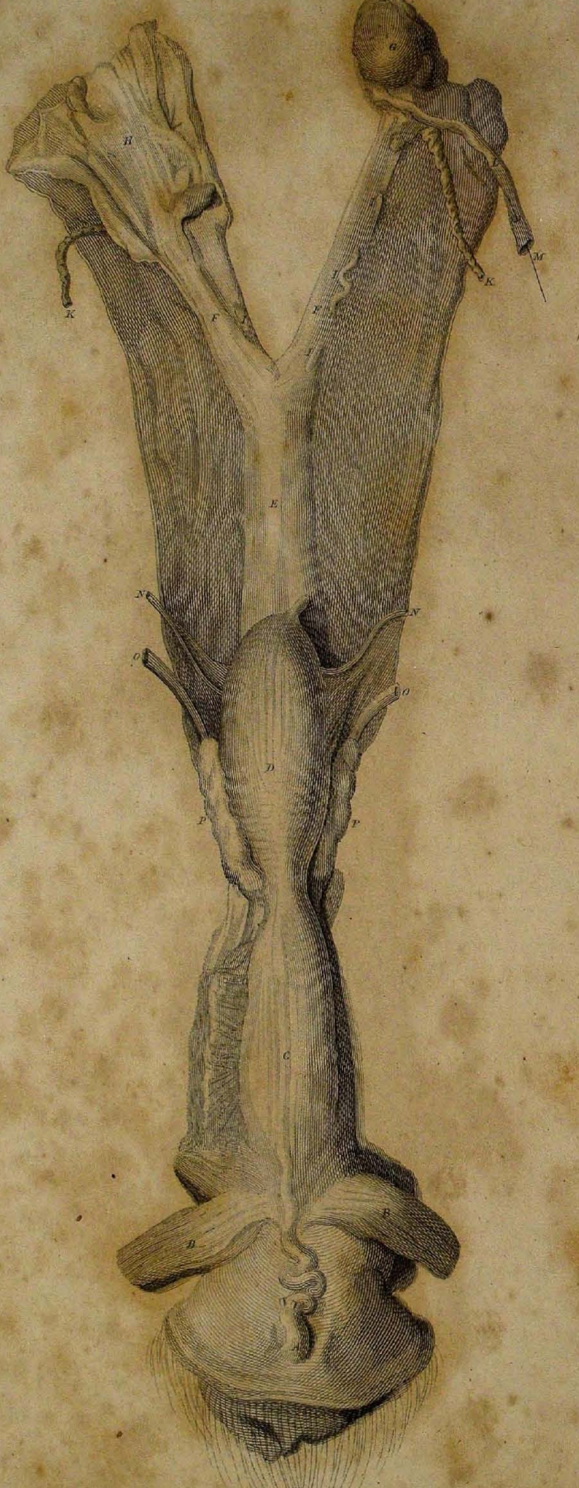
Wm. Skelton sculp.

PLATE X.

THIS plate ſhows the organs of generation of Mr. Arbuthnot's free martin, which are almoſt a complete mixture of the male and female: with this ſtructure of the parts, the external appearances and general character of the animal correſponded, it being neither that of the bull nor cow, but a mixt character.

A The peak of the labia.
B B The two labia.
C The glans clitoridis.
D D The inſide of the common vagina.
E E Orifices of the ducts of two glands.
F The orifice of the meatus urinarius.
G G The true vagina.
H H Either the contracted vagina, or what may be called uterus.
I I The horns of ditto, only pervious a little way.
K The right ovarium deprived of its capſula.
L The left ovarium incloſed in its capſula.
M A briſtle introduced through the orifice into the capſula.
N The right teſticle.
O O O O The right vas deferens.
P P The veſiculæ ſeminales.
Q Q The ducts of veſiculæ ſeminales ſeen through the vagina.
R Points to the openings of the vaſa deferentia and veſiculæ ſeminales.

Plate XI.



Bell del.

Skelton sculp.

PLATE XI.

THIS plate exhibits a front view of the organs of generation of Mr. Wells's free martin, which are more the parts of a cow than of a bull; and the animal itſelf reſembled a young heifer very much in its appearance.

A The clitoris.
B B The crura clitoridis.
C The urethra.
D The bladder.
E The body of the uterus beyond the bladder, which is impervious.
F F The horns of ditto, which are alſo impervious.
G The left ovarium deprived of its capſula.
H The capſula incloſing its ovarium.
I I I I Interrupted parts of the vaſa deferentia.
K K The ſpermatic veſſels.
L The gubernaculum of the right ſide.
M The beginning of the tunica vaginalis communis, into which is introduced a briſtle to ſhow that it is hollow.
N N Veſſels going behind the bladder.
O O The two ureters.
P P The veſiculæ ſeminales.

AN ACCOUNT OF AN EXTRAORDINARY PHEASANT.

EVERY deviation from that original form and ſtructure which gives the diſtinguiſhing character to the productions of Nature, may not improperly be called monſtrous. According to this acceptation of the term, the variety of monſters will be almoſt infinite; and, as far as my knowledge has extended, there is not a ſpecies of animals, nay, there is not a ſingle part of an animal body, which is not ſubject to an extraordinary formation. Neither does this appear to be a matter of mere chance; for it may be obſerved, that every ſpecies has a diſpoſition to deviate from Nature in a manner peculiar to itſelf. It is likewiſe worthy of remark, that each ſpecies of animals is diſpoſed to have nearly the ſame ſort of defects, and to have certain ſupernumerary parts of the ſame kind: yet every part is not alike diſpoſed to take on a great variety of forms; but each part of each ſpecies ſeems to have its monſtrous form originally impreſſed upon it.

It is well known, that many orders of animals have the two parts deſigned for the purpoſe of generation different in individuals of the ſame ſpecies, by which they are diſtinguiſhed into male and female; but this is not the only mark of diſtinction; in the greateſt part, the male being diſtinguiſhed from the female by various other marks. The varieties which are found in the parts of generation themſelves, I ſhall call the firſt, or principal marks, being originally formed in them and belonging equally to both ſexes; all others depending upon theſe I ſhall call ſecondary, as not taking place till the firſt are becoming of uſe, and being principally, although not entirely, in the male.

One of the moſt general marks is, the ſuperior ſtrength of make in the male; and another circumſtance, perhaps equally ſo, is this ſtrength being directed to one part more than another, which part is that moſt

immediately employed in fighting. This difference in external form is more particularly remarkable in the animals whofe females are of a peaceable nature; as are the greateft number of thofe which feed on vegetables, and the marks to difcriminate the fexes are in them very numerous. The males of almoft every clafs of animals are probably difpofed to fight, being, as I have obferved, ftronger than the females; and in many of thefe there are parts deftined folely for that purpofe, as the fpurs in the cock, and the horns in the bull; and on that account, the ftrength of the bull lies principally in his neck, that of the cock in his limbs.

In carnivorous animals, whofe prey is often of a kind which requires ftrength to kill, we do not find fuch a difference in the form of the male and female; very little being difcernible in the dog and bitch; in the he or fhe cat; or in the cock and hen of the eagle. A difference, however, is often perceiveable in the whole or in fome part of their external covering; the mane of the lion diftinguifhing him from the lionefs: and the males of fuch animals as neither fight nor feed on flefh, being only diftinguifhable from the female by fome peculiarity in the covering of their bodies, as the cock and hen in many birds. The male of the human fpecies is diftinguifhed from the female, both by his general ftrength and his covering, as alfo by a difference of voice.

In thefe orders of animals whofe fexes are diftinct, we may not only obferve the genital organs to be fubject to mal-conformation, as in any other part of the animal; but that an attempt is fometimes made to unite the two organs in the fame animal body, making what may be called, an unnatural hermaphrodite. In producing the unnatural hermaphrodite, the fame laws feem to operate as in the mal-conformation of other parts of animals; it being obfervable, that thefe deviations obtain through a whole fpecies precifely in the fame manner. I have already given an account of the free martin, which exhibits a mixture of the two parts of generation in the fame animal.

It is my intention, at prefent, to extend my inquiry on this fubject no further than what relates to the refemblance which one fex bears to another, in thofe diftinguifhing properties which I term fecondary; for we find

that there is often a change of the natural properties of the female ſex into thoſe of the ſecondary of the male; the female, in ſuch caſes, now and then aſſuming the ſecondary peculiarities of the male. It is to be obſerved, that ſome claſſes are more liable than others to this change; a ſingular example of which is to be the ſubject of the following pages.

To bring the foregoing obſervations into one point of view, I here beg leave to remark, that in animals juſt born, or very young, there are no peculiarities to diſtinguiſh one ſex from the other, excluſive of what relates to the organs of generation, which can only be in thoſe who have external parts; and that towards the age of maturity, the diſcriminating changes beforementioned begin to appear; the male then loſing that reſemblance he had to the female in various ſecondary properties*; but that in all animals which are not of any diſtinct ſex, called hermaphrodites, there is no ſuch alteration taking place in their form when they arrive at that age. It is evidently the male which at this time in ſuch reſpects recedes from the female; every female being at the age of maturity more like the young of the ſame ſpecies than the male is obſerved to be: and if the male is deprived of his teſtes when young, he retains more of the original youthful form, and therefore more reſembles the female.

From hence it might be ſuppoſed, that the female character contains more truly the ſpecific properties of the animal than the male; but the character of every animal is that which is marked by the properties common to both ſexes, which are found in a natural hermaphrodite, as in a ſnail, or in animals of neither ſex, as the caſtrated male or ſpayed female.

But where the ſexes are ſeparate, and the animals have two characters, the one cannot more than the other be called the true; as the real diſtinguiſhing marks of each particular ſpecies, as has been mentioned above, are thoſe common to both ſexes; and which are likewiſe in the unnatural hermaphrodite. That theſe properties give the diſtinct character of ſuch animals is evident, for the caſtrated male and the ſpayed

* This is not common to all animals of diſtinct ſexes; for in fiſhes there is no great difference; nor in many inſects: nor in dogs, as has been already obſerved; however, it is conſiderable in many quadrupeds, but appears to be moſt ſo in birds.

female have both the ſame common properties; and when I treated of the free martin, which is a monſtrous hermaphrodite, I obſerved that it was more like the ox than the cow or bull; ſo that the marks characteriſtic of the ſpecies which are found in the animal of a double ſex, are imitated by depriving the individual of certain ſexual parts, in conſequence of which it retains only the true properties of the ſpecies.

They are curious facts in the natural hiſtory of animals, that by depriving either ſex of the true parts of generation, they ſhall ſeem to approach each other in appearances, and acquire a reſemblance to the unnatural hermaphrodite.

In ſome ſpecies of animals that have the ſecondary properties we have mentioned, there is a deviation from the general rules, by the perfect female, with reſpect to the parts of generation, aſſuming more or leſs the ſecondary character of the male.

This change does not appear to ariſe from any action produced at the firſt formation of the animal; and, in this reſpect, is ſimilar to what takes place in the male; neither does it grow up with the animal as it does to a certain degree in the male; but ſeems to be one of thoſe changes which happens at a particular period, ſimilar to many common and natural phenomena; like to what is obſerved of the horns of the ſtag, which differ at different ages; or to the mane of the lion, which does not grow till after his fifth year, &c.

This change has been obſerved in ſome of the bird tribe, but principally in the common pheaſant; and it has been obſerved by thoſe who are converſant with this bird, when wild, that there every now and then appears a hen pheaſant with the feathers of a cock: all, however, that they have deſcribed on the ſubject is, that this animal does not breed; and that its ſpurs do not grow. Some years ago, one of theſe was ſent to the late Dr. William Hunter, which I examined and found it to have all the parts peculiar to the female of that bird. This ſpecimen is ſtill preſerved in Dr. Hunter's muſeum.

Dr. Pitcairn having received a pheaſant of this kind from Sir Thomas Harris, ſhowed it as a curioſity to Sir Joſeph Banks and Dr. Solander. I

happening to be then preſent, was deſired to examine the bird; and the following was the reſult of my examination.

I found the parts of generation to be truly female: they being as perfect as in any hen pheaſant that is not in the leaſt prepared for laying eggs; and having both the ovaria and oviducts.

As the obſervations hitherto made have been principally upon birds found wild, little of their hiſtory can be known; but from what took place in a hen pheaſant, in the poſſeſſion of a friend of Sir Joſeph Banks, it appears probable that this change of character takes place at an advanced period of the animal's life, and does not grow up with it from the beginning. This lady, who had for ſome time bred pheaſants, and paid particular attention to them, obſerved that one of the hens, after having produced ſeveral broods, moulted; when the ſucceeding feathers were thoſe of a cock; and that this animal was never afterwards impregnated. Hence it is moſt probable, that all the hen pheaſants, found wild, having the feathers of a cock, were formerly perfect hens, but have been changed by age, or perhaps by certain conſtitutional circumſtances.

Having bought ſome pheaſants from a dealer in birds, among which were ſeveral hens, I perceived, the year after, that one of the hens did not lay, and that ſhe began to change her feathers. The year following ſhe had nearly the plumage of the cock, but leſs brilliant, eſpecially on the head; and it is more than probable that this was an old hen, nearly under circumſtances ſimilar to thoſe before deſcribed.

Lady Tynte had a favourite pyed pea-hen which had produced chickens eight ſeveral times; having moulted when about eleven years old, the lady and family were aſtoniſhed by her diſplaying the feathers peculiar to the other ſex, and appeared like a pyed peacock. In this proceſs the tail, which became like that of the cock, firſt made its appearance after moulting; and in the following year, having moulted again, produced ſimilar feathers. In the third year ſhe did the ſame; and, in addition, had ſpurs reſembling thoſe of a cock. She never bred after this change in her plumage, and died in the following winter during the hard froſt,

in the year 1775-6. This bird is now preſerved in the muſeum of the late Sir Aſhton Lever*.

From what has been related of theſe three birds, we may conclude, that this change is merely the effect of age, and obtains to a certain degree in every claſs of animals. We find ſomething ſimilar taking place even in the human ſpecies: for that increaſe of hair obſervable on the faces of many women in advanced life, is an approach towards the beard, which is one of the moſt diſtinguiſhing ſecondary properties of man.

Thus we ſee the ſexes which, at an early period, had little to diſtinguiſh them from each other, acquiring about the time of puberty ſecondary properties, which clearly characteriſe the male and female; the male at this time receding from the female, and aſſuming the ſecondary properties of his ſex.

The female, at a much later time of life, when the powers of propagation ceaſe, loſes many of her peculiar properties; and may be ſaid, except from mere ſtructure of parts, to be of no ſex; even receding from the original character of the animal, and approaching, in appearance, towards the male, or perhaps more properly towards the hermaphrodite.

* It might be ſuppoſed, that this bird was really a cock which had been ſubſtituted for the hen; but the following facts put this matter beyond a doubt. Firſt, there was no other pyed pea-fowl in the county. Secondly, the hen had nobs on her toes, which were the ſame after her change. Thirdly, ſhe was as ſmall after the change as before, therefore too ſmall for a cock. Fourthly, ſhe was a favourite bird, and was generally fed by the lady, and uſed to come for her food, which ſhe ſtill continued to do after the change in the feathers.

AN ACCOUNT OF THE ORGAN OF HEARING IN FISHES.

NATURAL hiſtory having ever been conſidered as worthy the attention of the curious philoſopher, has in all ages kept pace with the other branches of knowledge; and as both arts and ſciences have of late years been cultivated to a degree, perhaps, beyond what was ever known before, we find that natural hiſtory has not been neglected. All the nations of Europe appear ſolicitous to encourage the ſtudy; and in this iſland it has been purſued with more philoſophic ardour than was ever known in any country. It has become an object of purſuit to men poſſeſſed of affluent fortune; which they have not only dedicated to the cultivation of this ſcience, but have even riſked their health and lives in exploring unknown regions to increaſe the ſources of information, and in ſettling correſpondences every where, ſo as to bring materials into this country that might render it the ſchool of natural hiſtory. It is no wonder then, that a ſpirit of inquiry is diffuſed through almoſt all ranks of men; and that thoſe who cannot purſue it themſelves, yet chuſing at leaſt to benefit by the induſtry of others, are eager to be informed of what is already known.

Theſe reflections have induced me to publiſh this ſhort account of the Organ of Hearing in Fiſhes; for though the exiſtence of ſuch an organ is now known to many, it is ſtill a ſubject of diſpute with others, whether they poſſeſs the ſenſe or not.

Some time before I quitted my anatomical purſuits, in the year 1760, and went with the army to Belliſle, I had diſcovered this organ in fiſhes, and had the parts expoſed and preſerved in ſpirits. In ſome the canals were filled with coloured injection, which ſhowed them to great advantage; and in others were ſo prepared, as to fit them to be kept as dried

preparations*. My reſearches, in that and in every other part of the animal œconomy, have been continued ever ſince that time. I am ſtill inclined to conſider whatever is uncommon in the ſtructure of this organ in fiſhes, as only a link in the chain of varieties diſplayed in its formation in different animals, deſcending from the moſt perfect to the moſt imperfect, in a regular progreſſion†.

As in this age of inveſtigation, a hint that ſuch an organ exiſted would be ſufficient to excite a ſpirit of conjecture or inquiry, I was aware that there would not be wanting ſome men, who, whether they only imagined the fact might be true, or really found it to be ſo, would be very ready to aſſume all the merit of the diſcovery to themſelves. My attention was more ſtrongly called to this point by hearing, in converſation, that ſome anatomiſts in France, Germany, and Italy, had diſcovered the organ of hearing in fiſhes, and intended to publiſh on the ſubject. I therefore thought that it would be only juſtice to myſelf to deliver to the Royal Society, a ſhort account of that organ, a diſcovery of which I had made more than twenty years before. This account I ſhall reprint here, without adding any thing to what I had before written; reſerving a more complete examination of this ſubject for a larger work, on the ſtructure of animals, which I one day hope to have it in my power to publiſh.

I do not intend to give a full account of this organ in any one fiſh, or of the varieties in different fiſhes, but only of the organ in general; thoſe therefore who may wiſh to purſue this branch of the animal œconomy will think it deficient perhaps in the deſcriptive parts. If it was a difficult taſk to expoſe this organ in fiſhes, I ſhould perhaps be led to be more full in my deſcription of it; but in fact there is nothing more eaſy.

* I have injected theſe parts in other animals, both with wax and metals; which, the bone being afterwards corroded in ſpirit of ſea-ſalt, make elegant caſts of theſe canals.

† The preparations to illuſtrate theſe facts have been, ever ſince, ſhewn in my collection, to both the curious of this country and foreigners. In ſhewing whatever was new, or ſuppoſed to be new, the ears of fiſhes were always conſidered by me as one important article.

It may be proper juſt to obſerve here, that the claſs called ſepia has the organ of hearing, though ſomewhat differently conſtructed from what it is in fiſhes.

The organ of hearing in fiſhes is placed on the ſides of the ſkull, or cavity which contains the brain; but the ſkull makes no part of it, as it does in the quadruped and the bird; the organ being a diſtinct and detached part. In ſome fiſhes, as in thoſe of the ray kind, the organ is wholly ſurrounded by the parts compoſing the cavity of the ſkull; in others it is in part within the ſkill, or cavity which contains the brain, as in the ſalmon, cod, &c. the ſkull projecting laterally, and forming a cavity.

The organ of hearing in fiſhes appears to increaſe in dimenſions with the animal, and nearly in the ſame proportion; which is not the caſe with the quadruped, &c. the organs being in them nearly as large in the growing fœtus as in the adult. Neither is its ſtructure, by any degree, ſo complicated in fiſhes as in all thoſe orders of animals which may be reckoned ſuperior, ſuch as quadrupeds, birds, and amphibious animals; but there is a regular gradation from the firſt of theſe to fiſhes.

It varies in different genera of fiſhes; but in all, it conſiſts of three curved tubes which unite one with another; this union forms in ſome only one canal, as in the cod, ſalmon, ling, &c. and in others a tolerably large cavity, as in the ray kind. In the jack there is an oblong bag, or blind proceſs, which is an addition to theſe canals, and communicates with them at their union. In the cod, &c. this union of the three tubes ſtands upon an oval cavity; and in the jack there are two: the additional cavities in theſe fiſhes appearing to anſwer the ſame purpoſe with the cavity obſerved in the ray or cartilaginous fiſhes, which is at the union of the three canals.

The whole organ is compoſed of a kind of cartilaginous ſubſtance, very hard or firm in ſome parts, and in ſome fiſhes cruſted over with a thin bony lamella, to prevent it from collapſing; for as the ſkull does not form any part of theſe canals or cavities, they muſt be compoſed of a ſubſtance capable of keeping its form.

Each tube deſcribes more than a ſemicircle; reſembling, in ſome ſort, what we find in moſt other animals, but differing in the parts being diſtinct from the ſkull*.

Two of the ſemicircular canals are ſimilar to one another, may be called a pair, and are placed perpendicularly; the third is not ſo long, and in ſome is placed horizontally, uniting as it were the other two at their ends or terminations. In the ſkate, this is ſomewhat different, being only united to one of the perpendicular canals. The two ſemicircular canals, whoſe poſition is perpendicular, are united, forming one canal; at their other extremities they have no connection with each other, but join the horizontal one, near its entrance into the common cavity. Near the union of theſe canals they are ſwelled out into round bags, and become much larger.

In the ray kind all theſe canals terminate in one cavity; and in the cod, in one canal, placed upon the additional cavity or cavities, in which there is a bone or bones. In ſome there are two bones; and in the jack, which has two cavities, we find in one of them two bones, and in the other one; in the ray there is only a chalky ſubſtance†.

In ſome fiſhes the external communication, or meatus, enters at the union of the two perpendicular canals; which is the caſe with all the ray kind, the external orifice being ſmall, and placed on the upper flat ſurface of the head; but it is not every genus or ſpecies of fiſhes that have the external opening.

The nerves of the ear paſs outwards from the brain, and appear to terminate at once on the external ſurface of the enlarged part of the ſemicircular tubes above deſcribed. They do not appear to paſs through theſe tubes ſo as to get on the inſide, as is ſuppoſed to be the caſe in quadrupeds; I ſhould therefore very much ſuſpect, that the lining of the tubes in the quadruped is not nerve, but a kind of internal perioſteum.

* The turtle and the crocodile have a ſtructure ſomewhat ſimilar to this; and the intention is the ſame, for their ſkulls make no part of the organ.

† This chalky ſubſtance is alſo found in the ears of amphibious animals.

As it is evident that fiſhes poſſeſs the organ of hearing, it becomes unneceſſary to make or relate any experiment made with living fiſhes, which only tends to prove this fact; but I will mention one experiment, to ſhow that ſounds affect them much, and is one of their guards, as it is in other animals. In the year 1762, when I was in Portugal, I obſerved in a nobleman's garden, near Liſbon, a ſmall fiſh-pond full of different kinds of fiſh. The bottom was level with the ground, the pond having been made by forming a bank all round, and had a ſhrubbery cloſe to it. Whilſt I lay on the bank, obſerving the fiſh ſwimming about, I deſired a gentleman, who was with me, to take a loaded gun and fire it from behind the ſhrubs. The reaſon for deſiring him to go behind the ſhrubs was, that there might not be the leaſt reflection of light. The moment the report was made, the fiſh ſeemed to be all of one mind, for they vaniſhed inſtantaneouſly, raiſing a cloud of mud from the bottom. In about five minutes afterwards they began to appear, and were ſeen ſwimming about as before.

Monſ. Geoffroi, who has written on this organ, conſiders the ray as in the claſs of reptiles; and with that idea has examined their organ of hearing. He is by no means clear in his deſcription, ſo that it is almoſt impoſſible to follow him; yet it is but doing him juſtice to allow, that he has diſcovered what is analogous to the three ſemicircular canals in other animals, together with their union into one cavity. He mentions the chalky ſubſtance contained in that cavity, and alſo the nerves: but it is by no means clear, that he was acquainted with the external opening which leads to theſe canals. He ſays, "The entrance of the organ of hearing (by which one would ſuppoſe he means the meatus auditorius exturnus) is not eaſily diſcovered"; but that which he deſcribes does not correſpond with the real ſituation of the external communication; we may therefore reaſonably conclude, that he is deſcribing ſomething elſe. He is not more clear in his mode of reaſoning on the application of the parts to produce the ſenſe of hearing. He obſerves, that the organ of hearing is very imperfect in this ſpecies of animals; but ſuppoſes this to be com-

penſated, by the medium in which they live, and by which ſound is conveyed to them, being more denſe than that of the air, by which ſound is communicated to animals living on the land; and of this idea he is certainly the author. Monſ. Geoffroi cannot indeed be ſaid to have given a perfect account of the organ of hearing in fiſhes, yet on the whole he ſhould be conſidered as a diſcoverer: for though he only made his obſervations on the ray, as belonging to the claſs of reptiles, yet as it may be properly conſidered of the fiſh kind, he has a juſt claim to that credit. Had I formerly been acquainted with this author's reſearches and pretenſions, I ſhould not have claimed that to which I had not a prior right; nor ſhould I have held the diſcovery of the external communication alone, an object of conſequence enough to induce me to diſpute the honour with Monſ. Geoffroi.

In looking over the works of the different authors who have treated of the organ of hearing in fiſhes, I find from a paſſage in Willoughby,* who publiſhed prior to Monſ. Geoffroi, and indeed is quoted by him, that my claim, even to the diſcovery of the external opening, is not ſo ſtrong as I believed it to be; as he mentions an external orifice in the ſkate, contiguous to what he ſuppoſes the organ of hearing in that fiſh. If what he alludes to is really the external opening of the ear, it gives him a prior claim to the diſcovery of that part of the organ; although from his account, he does not ſeem to have been acquainted with the organ itſelf: for as in deſcribing the external ear of the thornback, he has evidently miſtaken the noſe of it, of which he gives a tolerable full account, it is very obvious that he was ignorant of the opening into the ear†.

Although profeſſor Camper publiſhed an account of the organ of hearing in fiſhes ſo late as 1774, he did not ſeem, at that time, to have been acquainted with the external opening of the ear in the ray. After giving a deſcription of the organ of hearing in the pike, he makes ſome general obſervations on the ſimilarity of this organ in other fiſhes; but excepts

* Willughbeii Hiſtoria Piſcium, Oxonii 1686, lib. iii. cap. viii.

† Lib. iii. cap. xiv.

the ſhark and ray*. This exception we might ſuppoſe alluded to the auditory canal; but further on he explains what is meant by this exception, and does not mention the external opening in the ray; from which we may fairly conclude that he was not acquainted with it†.

* "Il eſt très-vraiſemblable que toutes les autres eſpèces de poiſſons, tant *malacopterygie* qu' *acanthopterygii*, auſſi-bien que les *branchioſtegi* & les *chondropterygii* d'Artedi, à l'exception des ſqualis & des raies, ont l'organe de l'ouïe conſtruit à peu près de la même façon; je n'excepte pas l'eſturgeon, quoique M. Klein, *ibid.* ait donné la deſcription du conduit auditif, *page* 19, *figure A*, *Tab.* 2. *b*; ce poiſſon étant rare parmi nous, je n'ai eu occaſion de l'examiner qu'une ſeule fois ſans avoir trouvé ce conduit." Memoirs Etrangers de l'Academie des Sciences, 1774, tom. 6, page 190.

† "Au contraire, les chiens de mer, les *galeis* de Rondelet & les poiſſons qu'il a décrits, *lib. XII*; les *ſqualis* d'Artedi & les raies, ont bien l'organe à peu près de la même compoſition, mais il eſt enfermé dans une caiſſe tout eſſeuſe ou cartilagineuſe, ce qui ne fait pas une différence eſſentielle; ils entendent donc comme les églefins, les morues, les baudroyes & les brochets, en un mot comme tous les autres poiſſons non amphibies: M. Geoffroi s'eſt trompé en comparant leurs organes avec celui de reptiles, tels que la vipère, les lézards, &c. qui entendent le ſon comme les quadrupèdes, les oiſeaux & les amphibies aquatiques, ſavoir par le moyen de l'air & d'un tambour, comme j'ai deſſein de le prouver dans une autre occaſion." Memoires Etrangers de l'Acadamie des Sciences, 1774, tom. 6, page 190.

AN ACCOUNT OF CERTAIN RECEPTACLES OF AIR IN BIRDS, WHICH COMMUNICATE WITH THE LUNGS AND EUSTACHIAN TUBE.

SINCE the account of theſe receptacles was read before the Royal Society, in the year 1774, I have, by the diſſection of a number of birds, been able to make ſome additional obſervations relative to the extent of the air-cells which communicate with the lungs in animals of this claſs. Theſe latter obſervations were not, however, made in conſequence of any regular deſign to inveſtigate this ſubject further, as to have eſtabliſhed the principle ſeemed all that was neceſſary; unleſs by general obſervations we could hope to throw more light on the final intention of this remarkable piece of mechaniſm.

Before the period I have mentioned, the communication ſubſiſting in birds, between the air-cells of the lungs and other cavities of the body, had not been clearly explained, nor even much attended to by anatomiſts or natural hiſtorians. It is a ſingularity of ſtructure peculiar to this tribe of animals; and an account of it, cannot, I imagine, be unacceptable to the public.

It is not my preſent intention to enter into minute deſcriptions of all the particular communications of this ſort diſcoverable in birds by diſſection, but only to mention ſuch general facts as may ſerve to introduce the ſubject into natural hiſtory, and lead to an inquiry into the purpoſes which this ſtructure was intended to anſwer. With this view I ſhall endeavour to give ſome idea of the conſtruction of the lungs, and of the air-receptacles in birds; occaſionally remarking the circumſtances in which theſe principally differ from what is ſeen in other animals.

The mechaniſm of the lungs in birds, which renders them fit for conveying air to different parts of the body, conſiſts principally in certain communications.

It has been aſſerted that birds have no diaphragm; but this opinion muſt have ariſen either from a want of obſervation, or from too confined an idea of a diaphragm; for there is a moderately ſtrong, but thin and tranſparent membrane, covering the lower ſurface of the lungs, and adhering to them, that affords inſertion to ſeveral thin muſcles which ariſe from the inner ſurfaces of the ribs. The uſe of this part ſeems to be that of leſſening the concavity of the lungs towards the abdomen, at the time of inſpiration, and thereby aſſiſting to dilate the air-cells; for which reaſon it is to be conſidered as anſwering one main purpoſe of a diaphragm. Beſides this attachment of the lungs to the diaphragm, they are alſo connected to the ribs, and to the ſides of the vertebræ.

Such adheſions are peculiar to this tribe of animals, and are of ſingular uſe, nay in fact are abſolutely neceſſary in lungs like thoſe of birds; out of which it is intended the air ſhould find a paſſage into other cavities. For if the lungs were looſe in the cavity of the thorax, as is the caſe in many other animals, the cells of the lungs could not be expanded, either by the depreſſion of the diaphragm, or the elevation of the ribs; ſince the air ruſhing in to fill up the vacuum produced in the cavity of the cheſt, by theſe actions, would take the ſtraight road from the trachea through the paſſages, and of conſequence would expand no part of the lungs which lay out of that line, whereby reſpiration would be totally prevented, and an effect produced exactly ſimilar to what happens in other animals when the lungs are ſo much wounded as to allow a free exit to the air at that part.

The cells in the bodies of birds which receive air from the lungs, are to be found both in the ſoft parts, and in the bones; and have no communication with the cavity of the common cellular membrane. Some of theſe air-bags are placed in the larger cavities, as in the abdomen; and others are ſo lodged in the interſtices of muſcles, blood, veſſels, and nerves, about the breaſt, axilla, &c. as at firſt to give the appearance of the common connecting membrane. Some communicate immediately with one another; and all may be ſaid to have a communication by means of the

lungs. They are of very different ſizes, as may beſt ſuit the particular circumſtances of the parts in which they are placed.

The bones which receive air are of two kinds; ſome, as the ſternum, ribs, and vertebræ, having their internal ſubſtance divided into innumerable cells; whilſt others, as the os humeri and os femoris, are hollowed out into one large canal, with ſometimes a few bony columns running acroſs at its extremities. Bones of this kind may be diſtinguiſhed from thoſe that do not receive air, by ſeveral marks: firſt, By their leſs ſpecific gravity: ſecondly, By being leſs vaſcular than the others, and therefore whiter: thirdly, By their containing little or no oil, and conſequently being more eaſily cleaned; and when cleaned, appearing much whiter than common bones: fourthly, By having no marrow, or even any bloody pulpy ſubſtance in their cells: fifthly, By not being in general ſo hard and firm as other bones*; and ſixthly, By the paſſage that allows the air to enter the bones, which can be eaſily perceived. In the recent bone we may readily diſcover holes, or openings, not filled with any ſoft ſubſtance as blood-veſſels or nerves; ſeveral of theſe holes are placed together, near that end of the bone which is next to the trunk of the bird; and are diſtinguiſhable by having their external edges rounded off, which is not the caſe with the holes through which either nerves or blood-veſſels paſs into the ſubſtance of the bone. When birds break any of the bones which contain air, the ſurrounding parts often become emphyſematous.

There are openings in the lungs, by which air is tranſmitted to the other parts; and the membrane or diaphragm, abovementioned, is perforated in ſeveral places with holes of a conſiderable ſize, which admit of a free communication between the cells of the lungs and the abdomen, a circumſtance which has been frequently noticed. To each of theſe perforations is joined a diſtinct membranous bag, extremely thin and tranſparent; which bags being afterwards continued through the whole of the abdomen and attached to the back and ſides of that cavity, are kept firm

* The bones of ſome birds are ſo ſoft that they can be ſqueezed together with the finger and thumb; the bones of the extremities, however, have very ſolid ſides.

in their proper ſituations; each receiving the air from their reſpective openings. There is no occaſion to deſcribe here all the bags, or their attachments, it being ſufficient to have ſaid, that they extend over the whole abdomen.

The lungs at the anterior part, contiguous to the ſternum, have openings into certain membranous cells which lie upon the ſides of the pericardium, and communicate with the cells of the ſternum. At the ſuperior part the lungs have a communication with the large cells of a looſe net-work, through which the trachea, œſophagus, and great veſſels paſs as they are going to and from the heart. When theſe cells are diſtended with air, the ſize of that part where they lie is very conſiderably increaſed, and this enlargement is in general a mark of either the paſſion of anger or love. It is plainly ſeen in the Turkey-cock, the pouting pigeon, &c. and is very viſible in the breaſt of a gooſe when ſhe cackles. Theſe cells communicate with others in the axilla, under the large pectoral muſcle; and in ſome birds are ſtill further extended. In the pelican, for inſtance, the ſkin of the whole body, even to the tip of the wing, is united to the part underneath, by means of theſe cells, which are equally formed; and when the ſkin is removed, the two ſeparated ſurfaces appear as if honey-combed. When the cells are diſtended, the ſkin is removed to a conſiderable diſtance, by which means the volume is proportionally increaſed. In moſt birds, I believe in all that fly, theſe axillary cells communicate with the cavity of the os humeri, by means of ſmall openings in the hollow ſurface near the head of that bone; in ſome they are continued down the wing, communicating with the ulna and radius; in others they reach even as far as the pinions. The oſtrich, however, is an exception.

The poſterior edge of the lungs (which lies on the ſides of the ſpine and projects backwards between the ribs) communicates with the cells of the bodies of the vertebræ, with thoſe of the ribs, the canal of the medulla ſpinalis, and the cells of the ſacrum, and other bones of the pelvis; from which parts the air finds a paſſage into the cavity of the thigh-bone. This takes place in the greateſt number of birds; but in ſome the air is even continued part of the way down the thighs. This account

agrees with what we generally find, though ſome birds have more, and ſome fewer of theſe communications; for, in the oſtrich, no air gets into the os humeri, yet it enters into every other part, as before deſcribed, and in very large quantities. In the common fowl no air appears to enter any bone except the os humeri. The wood-cock has no air-cells, either in the firſt bones of the wing, or in the thigh bones. On the other hand, in the pelican the air paſſes on to the ulna and radius, and into thoſe bones which anſwer to the carpus and metacarpus of quadrupeds.

Thus the cells of the abdomen, thoſe ſurrounding the pericardium, thoſe ſituated at the lower and forepart of the neck, and in the axilla, thoſe in the cellular membrane under the pectoral muſcles, as well as in that which unites the ſkin to the body, all communicate with the lungs, and are capable of being filled with air; and again from them, the cells of the ſternum, ribs, vertebræ of the back and loins, bones of the pelvis, the humeri, the ulna and radius, with the pinions and thigh bones, can in many birds be furniſhed with air.

It is not by the lungs alone, that air is conveyed into the bones of birds; for the cells of the diploe between the two plates of the ſkull, in ſome birds, receive a conſiderable quantity of air by the Euſtachian tube.* Of this the owl is a remarkable inſtance. The lower jaw of ſome kinds, is likewiſe ſupplied with air, and often by the ſame canal.† Some authors have conſidered the deploe in the cranium of a bird as a continuation

* The only thing, in other animals, ſimilar to this communication in birds, of the cells of bones with the external air, is that which takes place in the internal ear of quadrupeds, by means of the Euſtachian tube.

† When I wrote this account to ſend it to the Royal Society, I did not then know by what means this was done; for in that I ſaid, "but by what means I do not know"; that is, I did not know whether it was conveyed by the trachea, where it paſſes along the neck, or the Euſtachian tube. Profeſſor Camper, when he did me the honour to call upon me, was ſo obliging as to take ſome pains to ſhow me, in the lower jaw of the hawk, the hole where the air entered; which makes me ſuſpect he did not underſtand what I had written. For after having given the marks by which ſuch openings were particularly diſtinguiſhed, it will hardly be ſuppoſed I could ſay that I did not know the hole where the air entered.

of the mamillary procefs; and looked upon it as a circumftance peculiar to finging birds; which is not really true.

Thefe facts, which had been formerly obferved, led me in the year 1758 to make feveral experiments upon the breathing of birds, that might prove the free communication between the lungs and the before mentioned parts.

Firft, I made an opening into the belly of a cock, and having introduced a filver cannula, tied up the trachea; I found that the animal breathed by this opening, and might have lived; but by an inflammation in the bowels coming on, adhefions were produced, and the communication was cut off.

I next cut the wing through the os humeri, in another fowl, and tying up the trachea, as in the cock, found that the air paffed to and from the lungs by the canal in this bone. The fame experiment was made with the os femoris of a young hawk, and was attended with a fimilar refult. But the paffage of air through the divided parts, in both thefe experiments, efpecially in the laft, was attended with more difficulty than in the former one; it was indeed fo great, as to render it impoffible for the animal to live longer than evidently to prove that it breathed through the cut bone.

I have made feveral preparations of thefe cells, by throwing into the trachea an injection, commonly called the corroding injection, which firft filled the air-cells of the lungs, then all the others, fuch as the cells in the abdomen, anterior and fuperior part of the cheft, axilla, os humeri, cells of the back-bone and thigh; and the whole being afterwards put into fpirit of fea-falt, and corroded, the caft of injection came out entire.

The fingularity of thefe communications in birds, put me upon confidering what might be their final intention. At firft I fuppofed it might be intended to affift the act of flying, that being the circumftance which appears the moft peculiar to birds; and it might be of fervice in that refpect, I thought, by increafing the volume and ftrength with the fame quantity of matter, without adding to the weight of the whole, which indeed would rather be diminifhed by the difference of fpecific gravity be-

tween the external and internal air. This opinion was ſtrengthened, by diſcovering that the feathers of birds contained alſo a conſiderable quantity of air, in the very part which requires the greateſt ſtrength; and by the analogy which is obſerved between this mechaniſm in birds, and what is diſcoverable in moſt kinds of fiſhes. For theſe laſt have air contained within their bodies, which I believe is commonly ſuppoſed to leſſen their ſpecific gravity, although this does not appear ſo neceſſary in fiſhes, who move in a much heavier element than birds.* But when I found that

* When we conſider that the elevating and ſuſpending apparatus is much ſmaller in fiſhes than in birds, we may reaſonably conceive the air in them was intended as a kind of equilibrium between the fiſh and water; and that progreſſive motion was the only thing wanted in the actions of fiſhes. Were we to reaſon upon general principles alone, we ſhould ſuppoſe that thoſe fiſhes who have the largeſt air-bags ſhould have their muſcles of a greater ſpecific gravity; and thoſe fiſhes that have none, ſhould have the lighteſt fleſh; therefore that the fleſh of the ſalmon and cod, which have an air-bag, ſhould be heavier than that of the ſhark, which has none: but to know how far this, which appeared to be reaſonable, was a fact, I made the following experiments:

Experiment 1. I took a portion of muſcle of the ſhark, cod, and ſalmon, of the ſame weight in air; and firſt examined how far they occupied the ſame ſpace, by immerſing them in water, and obſerving the riſe or fall of the water upon each of them being ſeparately immerſed in it.

The ſhark occupied the ſmalleſt ſpace, the ſalmon a little more, and the cod the largeſt.

Experiment 2. I then ſuſpended the ſame three portions, upon a level, in a glaſs veſſel filled with water about two feet high, and let them all go at the ſame inſtant, to ſee which would fall through the water in the ſhorteſt ſpace of time. The ſhark got to the bottom firſt, the ſalmon next, and the cod laſt.

It is neceſſary to obſerve that, in both theſe experiments, the difference in bulk, and in the times of their falling was very little; but, however, ſufficient to aſcertain the fact for which the experiments were inſtituted.

To ſee how far the muſcular fleſh of birds was ſpecifically lighter than that of a quadruped, I repeated the above experiments upon a portion of a hind, of a pigeon, and of a ſheep, but could diſcover no viſible difference in their weight.

It may be obſerved, there are in common two ſituations of oil in fiſhes; in one it is diffuſed through the fiſh, as if the body had been ſteeped in it, as in the ſalmon, herring, &c. In the other it is found in the liver, as in all of the ray kind, cod, &c. and in general thoſe that have it in one part have none in the other; however, there are ſome, although I believe but few, who

the oſtrich (which is not intended to fly) was amply provided with theſe cells; and that the common fowl, and many others of that claſs, which are endowed with the faculty of flying, were leſs liberally ſupplied: when I ſaw that even the wood-cock, which flies, and is ſuppoſed a bird of paſſage, was inferior in this reſpect to the oſtrich; and that the bat differed not in ſtructure from animals that do not fly, I was compelled, by ſo many contradictions to theory, to ſuppoſe that this ſingular mechaniſm might be intended for ſome other purpoſe.

The next conjecture that offered, was, that theſe cells were to be conſidered as an appendage to the lungs; and to this I was led by the analogy obſervable between birds and amphibious animals. For although both in the bird and amphibious tribe, as the ſnake, viper, and many others, the lungs are continued down through the whole belly, in form of two bags, and therefore appear to be larger than the lungs in any other animal, yet in all of them the quantity of ſurface expoſed to the air is much leſs than in the quadruped; for the cells of the lungs in the bird are larger, and in the ſnake, &c. the upper part only can perform the office of reſpiration with any degree of effect; the lower having comparatively but few air-veſſels. The air muſt paſs through this upper part before it gets to the lower in inſpiration, and muſt alſo repaſs in expiration, ſo that the reſpiratory ſurface has more air applied to it than what the lungs of themſelves could contain. It is not however to be ſuppoſed, that the air can be made to paſs to and fro in bones as in parts which admit of contraction and dilatation; the purpoſe anſwered by theſe bony cells muſt therefore be different; and perhaps they ſhould be conſidered as reſervoirs of air.* There is in fact a

who have their oil in form of what may be called fat, viz. in fleaks in the interſtice of parts, as the ſturgeon. The liver, in thoſe of the ray kind, is large and extended through the belly; therefore it might be ſuppoſed to lighten the body, from oil being lighter than water or the fleſh; but we have oil in the liver of the cod; and in the ſalmon and herring, the oil is diffuſed through the whole; therefore I am affraid we are not yet acquainted with the full effect of the air-bladder in fiſhes.

* It is not to be ſuppoſed that, the air in the cells in birds will be changed while flying; only accumulated and retained; not in the leaſt influenced by either inſpiration or expiration.

great ſimilarity between birds and that claſs of animals called amphibious; and although a bird and a ſnake are not the ſame in the conſtruction of the reſpiratory organs, yet the circumſtance of the air paſſing in both beyond the lungs, into the cavity of the abdomen, naturally leads us to ſuppoſe, that a ſtructure ſo ſimilar is deſigned in each to anſwer a ſimilar purpoſe. This analogy is ſtill further ſupported by the lungs in both conſiſting of large cells. Now in amphibious animals, the uſe of ſuch a conformation of the lungs is evident; as it is in conſequence of this ſtructure that they require to breathe leſs frequently than others: and in this reſpect it may, in birds, have ſome connection with flying; as that motion might eaſily be imagined to render frequency of reſpiration inconvenient, and a reſervoir of air would therefore become ſingularly uſeful. Although we are not to conſider this ſtructure in birds to be an extention of lungs, yet I can eaſily conceive this accumulation of air to be of great uſe in reſpiration. For, it was obſerved before, reſpecting the amphibii, that the air in its paſſage to and from theſe cells, muſt certainly have a conſiderable effect upon the blood in the lungs, by allowing a much greater quantity of air to paſs in a given time than if there was no ſuch conſtruction of parts;* and this opinion will not appear to be ill founded, if we conſider that both in the bird and the viper, the ſurface of the lungs is ſmall in compariſon to what it is in many other animals which have not

piration. It might be aſked, Where is the ſtricture upon the air when flying, ſo as to keep the parts diſtended; is it upon the outlets from the lungs, or is it at the glottis, as in the quadruped? For we may obſerve, that when an animal is uſing conſiderable exerciſe, it never either expands the lungs, nor makes a full expiration, giving the ribs and diaphragm as little extent of motion as poſſible, ſo that the body may be kept firm, which obliges it to breathe oftener; and as this quantity of air is not ſufficient for the accelerated motion of the blood, the animal gets what is called, out of breath; which is no more than the two not being proportioned; and when it reſts, it breathes as quick, and takes as long ſtrokes as poſſible, to make up the loſs. So that in exerciſe we probably breathe leſs air.

* It may, perhaps, occur to ſome that, the whole of theſe communicating cells are to be conſidered as extended lungs; but I can hardly think that any air which gets beyond the veſiculated lungs themſelves is capable of affecting the blood of the animal, as the other cavities into which it enters, whether of the ſoft parts or of the bones, appear to be very little vaſcular.

this extenſion of cavity. It is alſo a corroborating circumſtance, that in the fowl the air might have paſſed by a much readier way than through the lungs, into all the cells about the breaſt, neck, axilla, wings, &c. as theſe could have been filled from the lower end of the trachea, upon which many of them lie. But the air muſt now take a roundabout paſſage both in its way in and its way out, thoſe openings being upon the exterior ſurface of the lungs. We muſt not however give up the idea of ſuch ſtructure being of uſe in flying; for I believe we may ſet it down as a general rule, that in the birds of longeſt and higheſt flight, as eagles, this diffuſion of air is extended further than in the others. This opinion is ſtrengthened by comparing the ſtructure above deſcribed with the reſpiratory organs in the flying inſects, which are compoſed of cells diffuſed through the whole body: theſe are extended even into the head and down the extremities, while there is no ſuch appearance in the inſects that do not fly, as the ſpider: but why the pelican ſhould be ſo amply provided, I cannot ſay, not knowing the natural hiſtory of that bird ſufficiently to be able to judge of this point. Do they carry weights in the large fauces ſo great as to require ſuch an increaſe of ſubſtance without increaſe of weight?

How far this conſtruction of the reſpiratory organs may aſſiſt birds in ſinging, deſerves inveſtigation; as the vaſt continuance of ſong, between the breathings, in a Canary-bird, would appear to ariſe from it. This is a ſubject however which I ſhall not at preſent enter upon.

EXPERIMENTS AND OBSERVATIONS ON ANIMALS, WITH RESPECT TO THE POWER OF PRODUCING HEAT.

SOME late ingenious experiments and obſervations, publiſhed in the Philoſophical Tranſactions, upon a power which animals ſeem to poſſeſs of generating cold, induced me to look over my notes, containing ſome which I had made in the year 1766, indicating an oppoſite power in animals, whereby they are capable of reſiſting any external cold while alive, by generating within themſelves a degree of heat ſufficient to counteract it. Thoſe experiments were not originally inſtituted with any expectation of the event which reſulted from them, but for the purpoſe of ſatisfying myſelf, whether an animal could retain life after being frozen, as has been confidently aſſerted both of fiſhes and ſnakes. For that theſe, after being frozen, ſtill retain ſo much of life, as when thawed to reſume their vital actions, is a fact ſo well atteſted, that we are bound to believe it; and had my experiment ſucceeded, it was my intention to have tried the effects of freezing on living animals to a much greater degree than can ever happen accidentally.

I mention theſe circumſtances, to account for what might otherwiſe be attributed to negligence and inattention; namely, the little nicety that was obſerved in meaſuring the preciſe degree of cold applied in the experiments. Accuracy in this particular was not aimed at, being of no conſequence in the inquiry more immediately before me. The cold was firſt produced by means of ice and ſnow with ſal ammoniac or ſea-ſalt, to about the 10° of Fahrenheit's thermometer: ice was then mixed with ſpirit of nitre; but what degree of cold was thus produced I did not examine. This cold mixture was made in a tub ſurrounded with woollen cloths, and covered with the ſame, to prevent the effects of the heat of the atmoſphere upon the mixture itſelf, and to preſerve as much as poſſible a

cold atmoſphere within the veſſel. Animal juices, as the blood, freeze at 25°; ſo that a piece of dead fleſh could be frozen in an atmoſphere, cooled to that point.

EXPERIMENTS.

Experiment I. was made on two carp. Theſe were put into a glaſs veſſel with common river water, which was placed in the freezing mixture. The water not freezing faſt enough to haſten that effect, as much cooled ſnow was added as to render the whole thick. The ſnow round the carp melting, we put in more freſh ſnow, which melting alſo, was repeated ſeveral times, till we grew tired, and at laſt left them covered up in the yard to freeze by the joint operation of the ſurrounding mixture and the natural cold of the atmoſphere. They were frozen at laſt, after having exhauſted the whole powers of life in the production of heat. That this was really the caſe, could not be known till I had completed that part of the experiment for which the whole was begun, viz. the thawing of the animals. It was done very gradually; but the animals did not, with flexibility, recover life; and while in this cold, ſhewed ſigns of great uneaſineſs by their violent motions. *N.B.* In ſome of theſe experiments, where air was made the conductor of the cold and heat, that the heat might be more readily carried off from the animal, a leaden veſſel was uſed. It was ſmall for the ſame reaſon; and as it was neceſſary for the animal's reſpiration that the mouth of the veſſel ſhould communicate with the open air, it was made deep, that the cold of the atmoſphere round the animal might not be diminiſhed too quickly by the warmth of the open air, which would have ſpoiled it as a conductor.

Experiment II. was upon a dormouſe; the veſſel in which it was confined being ſunk in the cold mixture almoſt to its edge. The atmoſphere round the animal ſoon cooled; its breath froze as it came from the mouth; an hoar-froſt gathered on its whiſkers, and on all the inſide of the veſſel; and the external points of the hair became covered with the ſame. While this was going on, the animal ſhewed ſigns of great uneaſineſs: ſometimes

it would coil itſelf into a round form, to preſerve its extremities, and confine its heat; and finding that ineffectual, would then endeavour to make its eſcape*. Its motions became leſs violent by the ſinking of the vital powers; its feet were at laſt frozen; but we were not able to keep up the cold a ſufficient time to freeze the whole animal, the hair being ſo bad a conductor of heat, that the conſumption was not more than the animal powers were capable of ſupporting.†

Experiment III. was made with another dormouſe; and taught by the failure of the laſt experiment, I took care that the hair ſhould not a ſecond time be an obſtruction to our ſucceſs. Having therefore firſt made the animal wet all over, that its heat might be more inſtantaneouſly carried off, it was put into a leaden veſſel, and the whole placed in the cold mixture as before. The animal ſoon gave ſigns of feeling the cold, by repeated attempts to make its eſcape; and the breath and water evaporating from its body being ſoon frozen, appeared like a hoar-froſt on the ſides of the veſſel, and on its whiſkers; but while the vigour of life laſted, it defied the approach of the cold. However, from the hair being wet, and thereby rendered a good conductor, there was a much greater conſumption of heat than in the former experiment; which haſtened on a diminution of the power of producing it. The animal dying, ſoon became ſtiff; and upon being thawed, was found quite dead.

Experiment IV. A toad being put into a veſſel with water, at ſuch a depth as not to cover its mouth, was placed in the mixture cooled to between 10° and 15°. The water froze ſo near to the body of the animal, as quite to incloſe it, but without deſtroying life; yet though not frozen, it hardly ever recovered the uſe of its limbs.

* This ſhows, that cold carried to a great degree rather rouſes than depreſſes the animal action; but it appears from many circumſtances and obſervations, that a certain degree of cold produces inactivity both in the living and ſenſative principle, which will be further illuſtrated hereafter.

† Theſe experiments were made in preſence of Dr. George Fordyce and Dr. Erwin, teacher of Chymiſtry at Glaſgow; the latter of whom came in accidentally in the middle of our operations.

Experiment V. was with a ſnail, which froze very ſoon, in a cold between 10° and 13°. Theſe two laſt experiments were made in the winter, when the living powers of the animals ſelected for the trial are very weak; they might have reſiſted the cold more ſtrongly in the ſummer. Why the animals mentioned in the above experiments died before they were frozen, while thoſe which are expoſed to the atmoſphere in very cold climates do not, is a point I ſhall not pretend to determine; not knowing the difference between the effects of a natural and an artificial cold. It may be accounted for, by ſuppoſing that the natural cold in climates in which animals are found frozen, is ſo intenſe as to produce congelation immediately, before the powers of life are exhauſted; at leaſt whether it is ſo or not is worthy of inquiry.

It appears from the above experiments; firſt, That moſt probably the animals were deprived of life before they were frozen: ſecondly, That there was an exertion or expence of animal power in reſiſting the effects of cold, proportioned to the neceſſity: thirdly, That this exertion was in proportion to the perfection of the animal, and the natural heat proper to each ſpecies, and to each age. This exertion might alſo perhaps depend in ſome degree on other circumſtances not hitherto obſerved: for from experiment II. and III. upon dormice, I find that in the animals, which are of a conſtitution to retain nearly the ſame heat in all temperatures of the air, it required the greateſt cold I could produce to overcome this reſiſting power; while by experiment IV. and V. on the toad and ſnail, whoſe natural heat is not always the ſame, but is altered very materially according to the external heat or cold, this power was exhauſted in a degree of cold not exceeding 10° or 15°: and the ſnail being the moſt imperfect of the two, its powers of generating heat appeared to be much the weakeſt.

That the imperfect animals will allow of a conſiderable variation in their temperature of heat and cold, is proved by the following experiments. The thermometor being at 45°, the ball was introduced by the mouth into the ſtomach of a frog, which had been expoſed to the ſame cold. It roſe to 49°. I then placed the frog in an atmoſphere made warm by heated water, where I allowed it to ſtay twenty minutes; and

upon introducing the thermometer into the ſtomach, it raiſed the quick-ſilver to 64°. To what degree the more imperfect animals are capable of being rendered hotter and colder at one time than another, I have not yet aſcertained; but the torpidity of theſe animals in our winter is probably owing to the great change wrought in their temperature by the external heat and cold. The cold in their bodies is to ſuch a degree, as in a great meaſure to put a ſtop, while it laſts, to the vital functions; while in warmer climates no ſuch effect is produced. This variety not only takes place in animals of different orders; but in ſome degree in the ſame animal at different ages, even according to the different age of the parts in the ſame animal: a young animal requires more warmth than one full grown; and although an animal is equally old in all its original parts, yet there are often new ones formed in conſequence of diſeaſes; and we find, that theſe new or young parts in animals are not ſo able to ſupport life as the old, at leaſt for ſome time: but as animals are of different ages, and the ſame animal is always growing older, and of courſe more and more perfect, they then become more capable of generating heat than when they were younger. This, however, has its limitations; for after a certain period they again loſe this power, and therefore require a leſs ſtrongly conducting medium, or warm atmoſphere.

This power of generating heat, ſeems to be a property in an animal while alive. In the more perfect animals it is to preſerve a ſtandard heat; and as they are moſt commonly in an atmoſphere colder than themſelves, they have moſt commonly occaſion to exert it; and it is therefore a power only of oppoſition and reſiſtance; for it is not found to exert itſelf ſpontaneouſly and unprovoked; but muſt always be excited by the energy of ſome external frigorific agent, or diſeaſe; yet it is natural to ſuch animals that this power ſhould be called forth; as will be obſerved by and by. It does not depend on the motion of the blood, as ſome have ſuppoſed, becauſe it likewiſe belongs to animals who have no circulation: and the noſe of a dog, which is always nearly of the ſame heat in all temperatures of the air, is well ſupplied with blood; although we muſt allow where this power is greateſt, the circulation is the quickeſt: neither can it be

ſaid to depend upon the nervous ſyſtem; for it is found in animals that have no brain or nerves. However, it muſt be allowed, that all that claſs who poſſeſs this power in the higheſt degree, have the largeſt brain, although this power is not in the leaſt in proportion to the quantity of brain in that claſs. It is moſt probable that it ariſes from ſome other principle; a principle ſo connected with life, that it can, and does, act independently of circulation, ſenſation, and volition; and is that power which preſerves and regulates the internal machine. This power of generating heat, is in the higheſt perfection when the body is in health; and in many deviations from that ſtate, we find that its action is extremely uncertain and irregular; ſometimes riſing higher than the ſtandard, and at other times falling much below it. Inſtances of this we have in different diſeaſes, and even in the ſame diſeaſe, within very ſhort intervals of time. A very remarkable one fell under my own obſervation, in a gentleman who was ſeized with an apoplectic fit; and while he lay inſenſible in bed, covered with blankets, I found that his whole body would, in an inſtant, become extremely cold in every part, continuing ſo for ſome time; and, as ſuddenly, would become extremely hot. While this was going on alternately, there was no ſenſible alteration in his pulſe for ſeveral hours.

Being ſatisfied of the foregoing fact, that animals had a power of generating heat, I purſued the ſubject ſtill further; not ſo much with a view to account for animal heat, as to obſerve the different phenomena, with the variations or difference in the heat in different animals. In the courſe of my experiments having found variations in the degree of heat and cold in the ſame experiment, for which I could not account, I ſuſpected that this might ariſe from ſome imperfection in the conſtruction of the thermometer. I mentioned to Mr. Ramſden my objections to the common conſtruction of that inſtrument, and my ideas of one more perfect in its nature, and better adapted to the experiments in which I was engaged. He accordingly made me ſome very ſmall thermometers, ſix or ſeven inches long, not above two-twelfths of an inch thick in the ſtem; having the external diameter of the ball very little larger than that of the ſtem, on which was marked the freezing point. The ſtem was embraced by

a ſmall ivory ſcale ſo as to ſlide upon it eaſily, and retain any poſition. Upon the hollow ſurface of this ſcale were marked the degrees which were ſeen through the ſtem. By theſe means the ſize of the thermometer was very much reduced, and it could be applied to ſoft bodies with much more eaſe and certainty, and in many caſes in which the former ones could not be conveniently uſed; I therefore repeated with it ſuch of my former experiments as had not at firſt proved ſatisfactory, and found the degrees of heat very different, not only from what I had expected, but alſo from what I had found by my former experiments with thermometers of the common conſtruction.

I have obſerved above, and find it ſupported by every experiment I have made on the heat and cold of animals, that the more perfect have the greater power of retaining a certain degree of heat, which may be called their ſtandard heat, and allow of much leſs variation than the more imperfect animals: however, it will appear from the three experiments which I am now going to relate, that many, if not all of the more perfect, are ſtill incapable of keeping conſtantly to one degree; but may be altered from their ſtandard heat, either by external applications, or diſeaſe. Theſe variations are much greater below that ſtandard than above it; the perfect animals having a greater power of reſiſting heat than cold, ſo that they are commonly near their ultimate heat. Indeed we do not want any other proof of a variation than our own feelings; being all ſenſible of heat and of cold, which ſenſations could not be produced without an alteration really taking place in the parts affected; and that alteration could not take place if they did not become actually warmer or colder. I have often cooled my hands to ſuch a degree, that I could warm them by immerſing them in water juſt pumped; therefore my hands were really colder than the pump-water.

An increaſe of abſolute heat muſt alter the texture or poſition of the parts, ſo as to produce the ſenſation which we call heat: and as that heat is diminiſhed, the texture or poſition of the parts is altered in a contrary way; and, when carried to a certain degree, becomes the cauſe of the ſenſation of cold. Now theſe effects could not take place in either caſe without an increaſe or decreaſe of abſolute heat in the part; heat there-

fore in ſome one of its different degrees muſt be preſent. I ſhall not in this place attempt to ſettle whether heat is a body or matter, or only a property of matter; which appears to me to be merely a difference in terms; for a property muſt belong to ſomething. When heat is applied to the ſurface of the body, the ſkin becomes in ſome degree heated according to the application; which may be carried ſo far as actually to burn the living parts: on the contrary, in a cold atmoſphere, a man's hand may become ſo cold as to loſe that ſenſation altogether, and change it for pain. Abſolute heat and cold may be carried ſo far as even to alter the ſtructure of the parts upon which the actions of life depend.

As animals being ſubject to variations in the degrees of heat and cold from external applications, are of courſe, in this reſpect, affected in ſome meaſure like inanimate matter: and therefore, as parts are elongated or recede from the common maſs, theſe effects more readily take place: for inſtance, all projecting parts and extremities, more eſpecially toes, fingers, noſes, ears, combs of fowls, particularly of the cock, are more readily cooled, and are therefore moſt ſubject to be affected by cold. Animals are not only ſubject to an increaſe and decreaſe of heat, ſimilar to inanimate matter; but the tranſition from one to the other (as far as they admit of it) is nearly as quick. I ſhall not however confine myſelf to ſenſation alone, as that is in ſome degree regulated by habit: for a habit of uniformity in the application of heat and cold to an animal body, renders it more ſenſible of the ſmalleſt variation in either; while by the habit of variety it will become, in a proportional degree, leſs ſuſceptible of all ſuch ſenſations. This is proved every day, in cold weather, by people who are accuſtomed to clothe themſelves warm. In them the leaſt expoſure to cold air, although the effect produced in the ſkin is perhaps not the hundredth part of a degree, immediately gives the ſenſation of cold, even through the thickeſt covering: thoſe, on the contrary, who have been uſed to go thinly clothed, can bear the variation of ſome degrees without being ſenſible of it: of this the hands and feet afford an inſtance in point: exciting the ſenſation of cold when applied to another part of the body, without having before given to the mind an impreſſion of cold exiſting in theſe parts themſelves. The projecting

parts and the extremities are thofe which admit of the greateft change in their degrees of heat and cold, without materially affecting the animal, or even its fenfations. I find that by heat or cold externally applied to fuch parts, the thermometer may be made to rife or fall; but not in an equal proportion as when applied to inanimate matter. Nor are the living parts cooled or heated in the fame proportion, as appears from the application of the thermometer to the fkin; for the cuticle is to be confidered as a dead covering, capable of receiving greater degrees of heat and cold than the living parts underneath; and as it might be fufpected that the whole of the variation was in this covering, to remove any fuch doubt I made the following experiments.

Experiment I. I placed the ball of the thermometer under my tongue, where it was perfectly covered by all the furrounding parts; and having kept it there for fome minutes, I found that it rofe to 97°; yet it rofe no higher by being continued there. I then took feveral pieces of ice, about the fize of walnuts, and put them in the fame fituation, allowing them only to melt in part, that the application of cold might be better kept up, occafionally fpitting out the water arifing from the folution. Having continued this for ten minutes, I found, on introducing my thermometer, that it fell to 77°; fo that the mouth at this part had loft 20° of heat. The thermometer gradually rofe to 97° again; but did not in this experiment fink fo low as it would have done in the hand, if a piece of ice had been held in it for the fame length of time. Perhaps the furface under the tongue being furrounded with warm parts, renders it next to an impoffibility to cool it below that degree: but I rather fufpect that fuch parts as the hand will allow of greater latitude in this refpect, from having infenfibly acquired the habit of varying the degree of cold, and becoming of courfe lefs fufceptible of its impreffions, and therefore lefs eafily excited.

As a further proof, that the more perfect animals are capable of varying their heat, in fome meafure, according to the external heat applied, I fhall adduce the following experiments made on the human fubject.

The mouth being a part fo frequently in contact with the external

atmoſphere in the action of breathing, whatever is put into it may be ſuppoſed to be influenced by that atmoſphere; this will always render an experiment made in that part, relative to heat and cold, ſomewhat uncertain. I imagined that the urethra would anſwer better, becauſe being an internal cavity it can only be influenced by heat and cold applied to the external ſkin of the parts. I imagined alſo, that whatever effects the application of heat and cold might have, they would ſooner take place in the urethra, as being a projecting part, than in any other part of the body; and therefore if living animal ſubſtance was in any degree ſubject to the common laws of matter in this reſpect, the urethra would be readily affected: to determine this I procured a perſon who allowed me to make ſuch experiments as I thought neceſſary.

Experiment II. I introduced the ball of my thermometer into the urethra about an inch; which having remained there about a minute, the quickſilver roſe to 92°: at two inches it roſe to 93°; at four inches to 94°; and when the ball had got as far as the bulb of the urethra, where it was ſurrounded by warm parts, the quickſilver roſe to 97°.

Experiment III. Theſe parts being immerſed for one minute in water, heated only to 65°, and the thermometer introduced about an inch and a half into the urethra, the quickſilver roſe to 79°; which was repeated ſeveral times with the ſame reſult. To diſcover if there were any difference in the quickneſs of the tranſition of heat and cold in living and dead parts, and to determine if the extent to which each would go, were likewiſe different, I procured a dead penis for the purpoſe of making the comparative experiments that follow; being clearly of opinion that all ſuch trials ſhould be as ſimilar as poſſible, except in thoſe points where the difference (if there is any) makes the eſſential part of the experiment.

Experiment IV. The heat of the penis of a living perſon, an inch and a half within the urethra, being found exactly 92°; and the dead one being heated to the ſame degree, I had both immerſed in the ſame veſſel, with the water at 50°, when by introducing the thermometers ſeveral different times, I was able to note the comparative quickneſs with which they cooled from 92°; and obſerved that the dead cooled ſooner

by two or three minutes; the living ſunk the quickſilver to 58°, and the dead to 50°: the thermometer, although continued there ſome time longer, fell no lower. I repeated this experiment ſeveral times, with the ſame reſult; although at one time there was a ſmall difference in the degrees of heat of the penis and alſo of the water; but the difference in the reſult was nearly proportional in all the three different trials, therefore the ſame concluſions may be drawn from them. In theſe laſt experiments very little difference was obſerved between the cooling of a dead and of a living part; a circumſtance which we cannot ſuppoſe to take place uniformly through the whole body; as in that caſe, living animals would always be of the ſame degree of heat with the atmoſphere in which they live. The ſubject of theſe experiments not chooſing to have the part cooled lower than 53° or 54°, prevented my obſerving if the powers of generating heat were exerted to a greater degree when the heat was brought ſo low as to threaten deſtruction; but by ſome experiments on mice, which will be related hereafter, it will appear that the animal powers are rouſed to exert themſelves in this reſpect when neceſſary.

Having found, from the above experiments, that parts of an animal were capable of being cooled below the common or natural heat; I proceeded to make others, with a view to aſcertain if the ſame parts were capable of being made much hotter than the ſtandard heat of animals. The experiments were made in the ſame manner as the former, only the water was now hotter than the natural heat of the animal.

Experiment V. The natural heat of the parts being 92°, they were immerſed for two minutes in water heated to 113°, and the thermometer being introduced as before, the quickſilver roſe to 100° and a half. This experiment I alſo repeated ſeveral times, but could not raiſe the heat of the penis beyond 100° and a half: this was probably owing to the perſon not being able at the time to bear the application of water warmer than 113° By way of compariſon, I made

Experiment VI. The living and dead parts being both immerſed in water, gradually made warmer and warmer from 100° to 118°, and continued in that heat for ſome minutes, the dead part raiſed the thermo-

meter to 114°, while the living raifed it no higher than 102° and a quarter. It was obferved, by the perfon on whom the experiment was made, that after the parts had been in the water about a minute, the water did not feel hot; but on its being agitated it felt fo hot that he could hardly bear it. Upon applying the thermometer to the fides of the living glans, the quickfilver immediately fell from 118° to about 104°, while it did not fall more than a degree when put clofe to the dead; fo that the living glans cooled the furrounding water to a certain diftance.*

Experiment VII. The heat of the rectum in the fame man was 98° and a half exactly.

In the fecond, third, fourth, fifth, and fixth experiments, an internal cavity, which is both very vafcular and fenfible, was evidently influenced by external heat and cold, though only applied to the fkin of the part; while, in the feventh experiment, another part of the fame body, where external heat and cold could make little or no impreffion, was of the ftandard heat. Although it will appear from experiment, that the rectum is not the warmeft part of an animal; yet, in order to determine how far the heat could be increafed by ftimulating the conftitution to a degree fufficient to quicken the pulfe, I repeated the feventh experiment after the man had eaten a hearty fupper and drank a bottle of wine, which increafed the pulfe from 73° to 87°, and yet the thermometer only rofe to 98° and a half.

Having formerly made experiments upon dormice during the fleeping feafon, with a view to fee if there were any alteration in the animal œconomy at that time, I found among my notes an account of fome which appear to our prefent purpofe: but to be more certain of the accuracy of the former experiments, I repeated them with my new thermometer.

* This might furnifh an ufeful hint refpecting bathing in water, whether colder or warmer than the heat of the body: for if intended to be either colder or hotter, it will foon be of the fame temperature with that of the body; therefore in a large bath, the patient fhould move from place to place; and in a fmall one, there fhould be a conftant fucceffion of water of the intended heat.

Experiment VIII. In a room, in which the temperature of the air was between 50° and 60°, a ſmall opening was made in the belly of a dormouſe, of a ſufficient ſize to admit the ball of my thermometer, which being introduced into the belly at about the middle of that cavity, roſe to 80°, and no higher.

Experiment IX. The mouſe was put into a cold atmoſphere of 15° above 0, and left there for fifteen minutes; after which the thermometer being introduced a ſecond time, it roſe to 85°.

Experiment X. The mouſe was again put into a cold atmoſphere for fifteen minutes; and the thermometer being introduced, the quick-ſilver at firſt roſe to 72° only, but gradually came up to 83°, 84°, and 85°.

Experiment XI. It was put a third time into the cold atmoſphere, and allowed to ſtay there for thirty minutes; the lower part of the mouſe at the bottom of the diſh, was almoſt frozen; the whole of the animal was numbed, and a good deal weakened. The thermometer being introduced, the heat was found to vary in different parts of the belly; in the pelvis, near the parts moſt expoſed to the cold, it was as low as 62°; in the middle, among the inteſtines, about 70°; but near the diaphragm it roſe to 80°, 82°, 84°, and 85°; ſo that in the middle of the body the heat had decreaſed 10°. Finding a variation in different parts of the ſame cavity in the ſame animal, I repeated the ſame experiments upon another dormouſe.

Experiment XII. Having brought a healthy dormouſe, which had been aſleep from the coldneſs of the atmoſphere, into a room in which there was a fire (the atmoſphere at 64°); I introduced the thermometer into its belly, nearly at the middle, between the thorax and pubis, and the quickſilver roſe to 74° or 75°; turning the ball towards the diaphragm, it roſe to 80°; and when I applied it to the liver, it roſe to 81° and a half.

Experiment XIII. The mouſe being placed in an atmoſphere at 20°, and left there half an hour; when taken out was very lively, even much more ſo than when put in. Introducing the thermometer into the lower

part of the belly, the quickſilver roſe to 91°; and upon turning it up to the liver, to 93°.

Experiment XIV. The animal being replaced in the cold atmoſphere at 30°, for an hour, the thermometer was again introduced into the belly; at the liver it roſe to 93°; in the pelvis to 92°; the mouſe continuing very lively.

Experiment XV. It was again put back into an atmoſphere, cooled to 19°, and left there an hour; the thermometer at the diaphragm was 87°; in the pelvis 83°; but the animal was now leſs lively.

Experiment XVI. Having been put into its cage, the thermometer being placed at the diaphragm, in two hours afterwards, was at 93°.

As I was unable to procure hedge-hogs in the torpid ſtate, to aſcertain their heat during that period, I got my friend, Mr. Jenner, ſurgeon, at Berkley, to make the ſame experiments on that animal, that I might compare them with thoſe in the dormouſe; and his account is as follow:

"Experiment I. In the winter the atmoſphere at 44°, the heat of a torpid hedge-hog, in the pelvis, was 45°, and at the diaphragm 48° and a half.

Experiment II. The atmoſphere 26°, the heat of a torpid hedge-hog, in the cavity of the abdomen, was reduced ſo low as 30°.

Experiment III. The ſame hedge-hog was expoſed to the cold atmoſphere of 26° for two days; and the heat of the rectum was found to be 93°; the wound in the abdomen being now ſo ſmall that it would not admit the thermometer.

A comparative experiment was made with a puppy, the atmoſphere at 50°; the heat in the pelvis, as alſo at the diaphragm, was 102°.

In ſummer, the atmoſphere at 78°, the heat of the hedge-hog, in an active ſtate, in the cavity of the abdomen, towards the pelvis, was 95°; at the diaphragm 97°."

We find from theſe experiments, that the heat of an animal is increaſed under the circumſtances of cold, whenever there are actions to be carried on for which heat is neceſſary.

In the experiments on the firſt dormouſe, the heat of the animal was

80° ; which is below the ſtandard heat of the actions of that animal; and after being put into the cold mixture, its heat was raiſed to 85°. In the ſecond dormouſe the heat was raiſed, by repeated experiments, from 75° to 93°. This queſtion naturally occurs here; Was the increaſe of heat in the animals generated to reſiſt the artificial cold, produced by placing them in a cold atmoſphere? Or was it owing to a wound having been made into the cavity of the abdomen, and an exertion of the animal powers being required to repair the injury; which exertion could not take place without the increaſed degree of heat? That it was in conſequence of the wound, appears evident from the experiment made upon the ſecond hedge-hog; for in an atmoſphere of 26° of heat, it was in a very torpid ſtate, and did not raiſe the thermometer higher than 30°; but after being wounded and put back into the cold, and kept there for two days, its heat in the rectum was 93°; and ſo far from being torpid, it was lively, and the bed in which it lay felt warm*.

Why the heat of the dormouſe ſhould be ſo low as 80°, in an atmoſphere of between 50° and 60°, is not eaſily accounted for, except as the effect of ſleep. But I ſhould very much ſuſpect, that ſleep, ſimply conſidered, is out of the queſtion; it being an effect that takes place in all degrees of heat and cold. In animals whoſe voluntary actions are ſuſpended by cold, that appears to produce its effect by acting in a certain degree as a ſedative, in conſequence of which the animal faculties are proportionably weakened, though they ſtill retain, even under ſuch circumſtances, the power of carrying on all the functions of life. Beyond this point cold ſeems to act as a ſtimulant, and rouſes the animal powers to action for ſelf-preſervation. It is more than probable, that moſt animals are in this predicament; and that there is a degree of cold, correſponding with every particular order of animals, by which, when applied, the voluntary actions muſt be ſuſpended.

* It is found from experiments, that the heat of an inflamed part is nearly the greateſt or ſtandard heat of the animal; it appearing to be a part of the proceſs of inflammation to raiſe the heat up to the ſtandard.

When a man is aſleep, he is colder than when awake; and I find, in general, that the difference is about one degree and a half, ſometimes leſs. But this difference in the degree of cold between ſleeping and waking, is not a cauſe of ſleep, but an effect; for many diſeaſes produce a much greater degree of cold in the animal, without giving the leaſt tendency to ſleep; therefore the inactivity of animals from cold, muſt be different from ſleep. Beſides, all the operations of perfect life, as digeſtion, ſenſation, &c. are going on in the time of natural ſleep, at leaſt in the perfect animals; but none of theſe operations are performed in the torpid ſtate of animals.

To ſee if the reſult of theſe experiments upon dormice was peculiar to that ſpecies, I wiſhed to repeat the ſame experiments upon common mice; for which purpoſe, in

Experiment XVII, I made uſe of one ſtrong and vigorous; and the atmoſphere being at 60°, I introduced the thermometer into the abdomen; the ball being at the diaphragm, the quickſilver was raiſed to 99°; but at the pelvis only to 96° and three quarters.

Here there was a real difference of about 9° between the dormouſe and the common, the dormouſe only raiſing it to 80°, in two animals of the ſame ſize, in ſome degree of the ſame genus, and at the ſame ſeaſon of the year, and the atmoſphere of nearly the ſame temperature.

Experiment XVIII. The ſame mouſe was put into a cold atmoſphere of 13°, for an hour, and then the thermometer was introduced as before; but the animal had loſt heat, for the quickſilver at the diaphragm was raiſed only to 83°, in the pelvis to 78°.

Here the real heat of the animal was diminiſhed 16° at the diaphragm, and 18° in the pelvis, while in the dormouſe it gained five degrees, but loſt upon a repetition.

Experiment XIX. In order to determine whether an animal that is weakened has the ſame powers, with reſpect to preſerving heat and cold, as one that is vigorous and ſtrong, I weakened a mouſe by faſting, and then introduced the ball of the thermometer into its belly; the ball being at the diaphragm, the quickſilver roſe to 97°; in the pelvis to 95°, being

two degrees colder than the ſtrong mouſe: the mouſe being put into an atmoſphere as cold as the other, and the thermometer again introduced, the quickſilver ſtood at 79° at the diaphragm, and at 74° in the pelvis.

In this experiment the heat at the diaphragm was diminiſhed 18°, in the pelvis 21°.

This greater diminution of heat in the ſecond, than in the firſt, we may ſuppoſe proportional to the decreaſed power of the animal, ariſing from want of food.

To determine how far different parts of other animals than thoſe already mentioned, were of different degrees of heat, I made the following experiments upon a healthy dog.

Experiment XX. The ball of the thermometer being introduced two inches within the rectum, the quickſilver roſe to 100° and a half. The cheſt of the dog was then opened, and a wound made into the right ventricle of the heart; and immediately on the ball being introduced, the quickſilver roſe to 101° exactly. A wound was next made ſome way into the ſubſtance of the liver; and the ball being introduced, the quickſilver roſe to 100° and three quarters. It was next introduced into the cavity of the ſtomach, where it ſtood exactly at 101°. All theſe experiments were made within a few minutes.

Experiment XXI. The thermometer was introduced into the rectum of an ox, and the quickſilver roſe exactly to 99° and a half.

Experiment XXII. This was alſo repeated upon a rabbit, and the quickſilver roſe to 99° and a half.

From experiments on mice, and upon the dog, it plainly appears, that every part of an animal is not of the ſame degree of heat; and hence we may reaſonably infer, that the heat of the vital parts of man is greater than either the mouth, rectum, or the urethra.

To determine how far my idea was juſt, that the heat of animals varied in proportion to their degree of perfection, I made the following experiments upon fowls, which I conſidered as one remove below what are commonly called quadrupeds.

Experiment XXIII. I introduced the ball of the thermometer ſuc-

ceſſively into the inteſtinum rectum of ſeveral hens, and found that the quickſilver roſe as high as 103°, 103° and a half; and in one of them to 104°.

Experiment XXIV. I made the ſame experiments on ſeveral cocks, and the reſult was the ſame.

Experiment XXV. To determine if the heat of the hen was increaſed when ſhe was prepared for incubation, I repeated the twenty-third experiment upon ſeveral ſitting or clucking hens; in one the quickſilver roſe to 104°; in the other to 103° and a half; 103°, as in the twenty-third experiment.

Experiment XXVI. I placed the ball of the thermometer under the ſame hen, in whoſe rectum the quickſilver was raiſed to 104°, and found the heat as great as in the rectum.

Experiment XXVII. Having taken ſome of the eggs from under the ſame hen, where the chick was about three parts formed, I broke a hole in the ſhell, and introducing the ball of the thermometer, found that the quickſilver roſe to 99° and a half. In ſome that were addled, I found the heat not ſo high by two degrees; ſo that the life in the living egg aſſiſted in ſome degree to ſupport its own heat.

Is the increaſe of three or four degrees of heat, which is the difference found between the fowl and the quadruped, for the purpoſe of incubation? The heat in the eggs, which was cauſed and ſupported by that of the fowls, was not above the ſtandard of the quadrupeds; and it would probably have been leſs, if the heat of the hen had not been ſo great.

Finding from the above experiments, that fowls were ſome degrees warmer than that claſs commonly called quadrupeds (although certainly leſs perfect animals) I choſe to continue the experiments upon the ſame principle; and made the following upon thoſe of a ſtill inferior order. The next remove from the fowl being what is commonly called the amphibious.

Experiment XXVIII. I introduced the thermometer into the ſtomach, and afterwards into the anus of a healthy viper, and the quickſilver roſe from 58° (the heat of the atmoſphere in which it was) to 68°; ſo that it was ten degrees warmer than the common atmoſphere.

Experiment XXIX. Having aſcertained the heat of the water in a pond, in which there were carp, to be 65° and a half, I took a carp out of this water, and having introduced the thermometer into its ſtomach, the quickſilver roſe to 69°; ſo that the difference between the water and the fiſh was only three degrees and a half.

Experiment XXX. The heat of the atmoſphere at 56°, ſome earth-worms were put into a glaſs veſſel, and a thermometer being immerſed among them, the quickſilver ſtood at 58° and a half.

This experiment was repeated, the atmoſphere at 55°, and the worms were found to be 57°.

Experiment XXXI. The atmoſphere at 54°, four black ſlugs were put into a ſmall veſſel, and a thermometer immerſed among them, ſtood at 55° and a quarter.

Experiment XXXII. The atmoſphere at 56°, three leaches were put into a ſmall glaſs veſſel, and a thermometer immerſed among them, ſtood at 57°.

This experiment was repeated, the atmoſphere at 54°, when the thermometer ſtood at 55° and a half.

To ſee how far the colder animals had a power of preſerving their ſtandard heat, when expoſed to ſevere cold, I made the following experiments.

Experiment XXXIII. A viper, whoſe heat was 68°, was put into a pan, and the pan into a cold mixture of about 10°; after remaining there about ten minutes, had its heat reduced to 37°. Being allowed to ſtay ten minutes longer, the mixture at 13°, its heat was reduced to 35°. It was continued ten minutes more in the mixture at 20°, and its heat was reduced to 31°, nor did it ſink lower; its tail beginning to freeze; and the animal now becoming very weak. It may be remarked, that it cooled much ſlower than many of the animals mentioned in the following experiments.

The frog being, in its ſtructure, more ſimilar to the viper than to either the fowl or fiſh, I made the following experiments on that animal.

Experiment XXXIV. I introduced the ball of the thermometer into

its ſtomach, and the quickſilver ſtood at 44°. I then put the frog into a cold mixture, and the quickſilver ſunk to 31°; the animal appeared almoſt dead, but recovered very ſoon: beyond this point it was not poſſible to leſſen the heat, without deſtroying the animal. But its decreaſe of heat was quicker than in the viper, although the mixture was nearly the ſame.

The next experiments were made on fiſhes.

Experiment XXXV. In an eel, the heat in the ſtomach, which at firſt was at 37°, ſunk, after it had been ſome time in the cold mixture, to 31°. The animal at that time appeared dead, but was found to be alive the next day.

Experiment XXXVI. In a ſnail, whoſe heat was at 44°, it ſunk, after it had been put into the cold mixture, to 31°, and then the animal froze.

Experiment XXXVII. Several leaches having been put into a bottle, and the bottle immerſed in the cold mixture, the ball of the thermometer being placed in the middle of them, the quickſilver ſunk to 31°; and by continuing the immerſion for a ſufficient time to deſtroy life, the quickſilver roſe to 32°, and then the leaches froze. In all theſe experiments, the animals when thawed were found dead.

Finding that animals of the imperfect claſſes will, without life being totally extinguiſhed, admit of their heat being reduced to that point at which the dead ſolids and fluids freeze; but if ſunk much below that, death muſt be the conſequence; I wiſhed therefore to be able to determine to what degree the heat of the animal could be raiſed.

Experiment XXXVIII. A healthy viper was placed in an atmoſphere heated to 108°, and allowed to ſtay ſeven minutes; when the heat of the animal, in the ſtomach and anus, was found to be 92° and a half; beyond which it could not be raiſed in the above ſtate of the atmoſphere. The ſame experiment was made upon frogs, with nearly the ſame reſult.

Experiment XXXIX. An eel, very weak, its heat at 44°, which was nearly that of the atmoſphere, was put into water heated to 65°, for fifteen minutes; and, upon examination, it was found of the ſame degree of heat with the water.

Experiment XL. A tench, whofe heat was 41°, was put into water at 65°; and left there ten minutes; the ball of the thermometer being introduced both into the ftomach and rectum, the quickfilver rofe to 55°. Thefe experiments were repeated with nearly the fame refult.

To determine whether life had any power of refifting heat and cold in inferior claffes of animals, I made comparative trials between living and dead ones.

Experiment XLI. I took a living and a dead tench, and a living and a dead eel, and put them into warm water; they all received heat equally faft; and when they were expofed to cold, both the living and the dead admitted the cold likewife with equal quicknefs.

I had long fufpected, that the principle of life was not wholly confined to animals, or animal fubftance endowed with vifible organization and fpontaneous motion; but fuppofed, that the fame principle might exift in animal fubftances, devoid of apparent organization and motion, when the power of prefervation was fimply required.

I was led to this opinion about twenty years ago, when bufied in making drawings of the growth of the chick in the procefs of incubation. I then obferved, that whenever an egg was hatched, the yolk (which is not diminifhed in the time of incubation) was always perfectly fweet to the very laft; and that the part of the albumen, which has not been expended on the growth of the animal, fome days before hatching, was alfo perfectly fweet, although both were kept in a heat of 103° in the hen's egg for three weeks, and in the duck's for four; but I obferved, that if an egg was not hatched, that egg became putrid in nearly the fame time with any other dead animal matter.

To determine from other tefts how far eggs poffeffed a living principle, I made the following experiments.

Experiment XLII. After having placed an egg in a cold about o, till it froze, I allowed it to thaw; by which procefs it was to be fuppofed the preferving powers of the egg muft be deftroyed. I then put this egg into the cold mixture, and with it one newly laid, and found the difference in

freezing was ſeven minutes and a half, the freſh one, ſo much longer time reſiſting the powers of cold.

Experiment XLIII. A new laid egg being put into a cold atmoſphere, fluctuating between 17° and 15°, took above half an hour to freeze; but when thawed and put into an atmoſphere at 25°, it froze in half the time. This experiment was repeated frequently with nearly the ſame reſult.

To aſcertain the comparative degree of heat between a living and a dead egg, and alſo to determine whether a living egg be ſubject to the ſame laws with the more imperfect animals, I made the following experiments.

Experiment XLIV. A freſh egg, and one which had been frozen and thawed, were put into the cold mixture at 15°; the thawed one ſoon came to 32°, and began to ſwell and congeal; the freſh one ſunk to 29° and a half, and in twenty-five minutes later than the dead one, it roſe to 32, and began to ſwell and freeze.

In this experiment, the effect on the freſh egg was ſimilar to that produced on the frog, eel, ſnail, &c. where life allowing the heat to be diminiſhed two or three degrees below the freezing point, afterwards reſiſted all further decreaſe; but the powers of life being expended by this exertion, the parts froze like any other dead animal matter.

From theſe experiments it appears, that a freſh egg has the power of reſiſting heat, cold, and putrefaction, in a degree equal to many of the more imperfect animals; and it is more than probable, this power ariſes from the ſame principle in both.

From the circumſtance of thoſe imperfect animals (upon which I made my experiments) varying their heat ſo readily, we may conclude, that heat is not ſo very eſſential to life in them as in the more perfect; although it be eſſential to many of the operations, or what may be called the ſecondary actions of life, ſuch as digeſting food*, and propagating the ſpecies, both which, eſpecially the laſt, requiring the greateſt powers

* How far this idea holds good with fiſhes, I am not certain.

an animal can exert. The animals which we call imperfect being chiefly employed in the act of digeſtion, we may ſuppoſe their degree of heat to be only what that action requires; it not being eſſentially neceſſary for the life of the animal that heat ſhould ever riſe ſo high in them as to call forth the powers neceſſary for the propagation of the ſpecies.* Whenever therefore theſe imperfect animals are expoſed to a cold ſo great as to weaken their powers, and diſable them from performing the firſt of theſe ſecondary actions, they in ſome meaſure ceaſe to be voluntary agents, and remain in a torpid ſtate during that extreme degree of cold which always occurs during ſome part of the winter in the countries they inhabit; and the food of ſuch animals not being in general produced in the cold ſeaſon, is a reaſon why this torpidity becomes in ſome meaſure neceſſary.

From the heat of ſuch animals ſinking to the freezing point, or even lower, and then becoming ſtationary; and the animal not being able to ſupport life in a much greater degree of cold for any length of time, we ſee a reaſon why they ſhould always endeavour to procure places of abode in the winter where the cold ſeldom ſinks to that point. We find toads burrowing, frogs living under large ſtones, ſnails ſeeking ſhelter under ſtones and in holes, and fiſhes having recourſe to deep water; the heat of all thoſe places being generally above the freezing point even in our hard-

* The hedge-hog may be called a truly torpid animal; and we find that its actual heat is diminiſhed when the actions are not vigorous. From a general review of this whole ſubject it would appear, that a certain degree of heat in the animal is neceſſary for its various œconomical operations, among which is digeſtion; and that neceſſary heat will be according to the nature of the animal, and, probably, the nature of the operations to be performed. A frog will digeſt food when its heat is at 60°, but not when at 35° or 40°; and it is very probable that, when the heat of the bear, hedge-hog, dormouſe, bat, &c. is reduced to 70°, 75°, or 80°, they loſe their power of digeſtion; or rather that the body, in ſuch a degree of cold, has no call upon the ſtomach. That animals, in a certain degree of heat, muſt always have food, is further illuſtrated by the inſtance of bees. The conſtruction of a bee is very ſimilar to a fly, a waſp, &c. A fly and a waſp can allow their heat to diminiſh, as in the fiſh, ſnake, &c. without loſing life, but a bee cannot; therefore a bee is obliged to keep up its heat as high as what we call its digeſtive heat, but not its propagating; for which purpoſe they provide againſt ſuch cold as would deprive them even of their digeſtive heat, if they had not food to preſerve it.

eſt froſts ; which are however ſometimes ſo ſevere, as to kill many whoſe habitations are not well choſen.

When the froſt is more intenſe and of longer ſtanding than common, or in countries where the winters are always ſevere, there is generally ſnow on the ground, and the water freezes: the advantage ariſing from theſe two circumſtances is great; the ſnow ſerving as a blanket to the earth, and the ice to the water*.

As all the experiments I ever made upon the freezing of animals, (with a view to ſee if it were poſſible to reſtore the actions of life when thawed) were tried upon whole ones; as I never ſaw life return by thawing†, and wiſhed to ſee how far parts were ſimilar to the whole in this reſpect; it being aſſerted, and with ſome authority, that parts of a man may be frozen, and afterwards recover, I made, for this purpoſe, the following experiments upon an animal of the ſame order as ourſelves.

In January 1777, I mixed ſalt and ice till the cold was about 0; in the ſide of the veſſel was a hole, through which I introduced the ear of

* Snow and ice are perhaps the worſt conductors of heat of any ſubſtance yet known. In the firſt place, they never allow their own heat to riſe above the freezing point, ſo that no heat can paſs through ice or ſnow when at 32°, by which means they become an abſolute barrier to all heat that is at or above that degree; hence the heat of the earth, or whatever ſubſtance they cover, is retained: but they are conductors of heat below 32°. Perhaps that power decreaſes in proportion as the heat decreaſes under that point.

In the winter 1776, a froſt coming on, the ſurface of the ground was frozen; but a conſiderable fall of ſnow fell, and continued ſeveral weeks: the heat of the atmoſphere, during the time, was often at 15°; but ſo little did the froſt affect the ground underneath, that the ſurface of the ground thawed, and the earth retained the heat of 34°, in which beans and peas will grow.

The ſame thing took place in a pond where the water was frozen on the ſurface to a conſiderable thickneſs; a large quantity of ſnow having fallen, and covered the ice, the heat of the water was preſerved; the ice thawed, and the ſnow, at its under ſurface, was found mixed with the water.

The heat of the water under the ſnow was at 35°, in which fiſhes lived very well.

It would be an attempt worthy the attention of the philoſopher, to inveſtigate the cauſe of the heat of the earth, upon what principle it is preſerved, &c.

† Vide Phil. Tranſ. for the year 1775, vol. LXV, part II, p. 446.

a rabbit; and to carry off the heat as faſt as poſſible, it was held between two flat pieces of iron that went further into the mixture. That part of the ear projecting into the veſſel became ſtiff, and when cut did not bleed; the part divided by the ſciſſars flying from between the blades like a hard chip.

The ear having remained in the mixture nearly an hour, ſoon thawed when taken out, began to bleed, and became ſo very flaccid, as to double upon itſelf, from loſing its natural elaſticity. When out of the mixture nearly an hour, it became warm, and this warmth increaſing to a conſiderable degree, it alſo began to thicken, in conſequence of inflammation, while the other ear continued in its uſual degree of cold. The day following the frozen ear was ſtill warm; and even two days after retained its heat and thickneſs, which continued for many days after.

About a week after this, the mixture being the ſame as in the former experiment, I introduced the ears of the ſame rabbit through the hole, and froze them both: the ſound one however froze firſt, probably from its being conſiderably colder at the beginning. When withdrawn they ſoon thawed, both ſoon became warm, and the freſh ear thickened as the other had done before.

Such a change in the parts does not always take place ſo quickly; for on repeating the experiment on the ear of another rabbit till it became as hard as a board, it was found to be longer in thawing than in the former experiment, and much longer before it became warm; in two hours however, it had acquired ſome degree of warmth; and on the day following was hot and thickened.

In the ſpring, 1776, I perceived that my cocks in the country had their combs ſmooth, with an even edge, and not ſo broad as formerly, appearing as if near one half of them had been cut off. Having inquired into the cauſe of this, my ſervant told me, that it had happened in that winter during the hard froſt. He having then obſerved, that the combs had in part dropped off: alſo, that the comb of one cock had entirely ſeparated; but this I did not ſee, as by accident he was burnt to death. I naturally imputed this effect to the combs having been frozen to ſo

great a degree during the ſevere weather, as to have the life of the part deſtroyed: to determine, therefore, by experiment, the ſolidity of this reaſoning, I made the following experiment.

I ſelected, for the purpoſe, a very large young cock, having a comb of conſiderable breadth, with deep ſerrated edges, the proceſſes of which were full half an inch long. My attempts to freeze the ſubſtance of the comb did not ſucceed; for that being thick and warm, reſiſted the effects of the cold, and only the ſerrated edges were frozen. The frozen parts became white and hard; and, when I cut off a little bit, did not bleed, nor did the animal ſhow any ſigns of pain. I next immerſed in the cold mixture one of his wattles, which was very broad and thin; it froze very readily, and upon thawing both the comb and wattle, they became warm, but of a purple colour, having loſt that tranſparency which remained in the other parts of the comb and in the other wattle. The wound in the comb now bled freely.

Both comb and wattle recovered perfectly in about a month. The natural colour returned firſt neareſt to the ſound parts, increaſing gradually till the whole had acquired a healthy appearance.

There was a very material difference in the effect between thoſe fowls, the ſerrated edges of whoſe combs I ſuſpected to have been frozen in the winter of 1765-6, for they muſt have dropped off. The only way in which I can account for this difference is, that in thoſe fowls the parts were kept ſo long frozen, that the unfrozen or active parts had time to inflame, and had brought about a ſeparation of the frozen parts, treating them exactly as dead, ſimilar to a mortified part; and that before they thawed, the ſeparation was ſo far completed as to deprive them of further ſupport.

As it is confidently aſſerted, that fiſhes are often frozen, and again return to motion; and as I had never ſucceeded in any of my trials of the kind upon whole fiſhes, I made ſome experiments upon particular parts, to which I was led by having found a material difference in the reſult of experiments made upon the whole, and on parts of the more perfect animals.

I froze the tail of a tench, as high as the anus, which became as hard as a board; when thawed, that part was whiter than common; and when it moved, the whole tail moved as one piece, and the termination of the frozen part appeared like the joint on which it moved.

On the ſame day I froze the tails of two gold fiſhes till they became as ſolid as a piece of wood. They were put into cold water to thaw, and appeared for ſome days to be very well; but that part of the tails which had been frozen had not the natural colour, and the fins of the tails became ragged. About three weeks after, a fur came all over the frozen parts; their tails became lighter, ſo that the fiſhes were ſuſpended in the water perpendicularly; they had almoſt loſt the power of motion; and at laſt died. The water in which they were kept was New River water, ſhifted every day, and in quantity about ten gallons.

I made ſimilar experiments upon an order of animals ſtill inferior, viz. common earth-worms.

I firſt froze the whole of an earth-worm as a ſtandard; when thawed it was perfectly dead.

I then froze the anterior half of another earth-worm; but the whole died.

I next froze the poſterior half of an earth-worm; the anterior half continued alive, and ſeparated itſelf from the dead part.

From ſome of theſe experiments it appears, that the more imperfect animals are capable of having their heat and cold varied very conſiderably, but not according to the degree of heat or cold of the ſurrounding medium in which they can ſupport life; for they can live in a cold conſiderably below the freezing point, and yet the living powers of the animal will not allow their heat to be diminiſhed much beyond 32°. Whenever the ſurrounding cold brings them ſo low, the power of generating heat takes place; and if the cold is continued, the animals exert this power till life is deſtroyed; after which they freeze, and are immediately capable of admitting any degeee of cold.

Plate XII.

Fig. I.

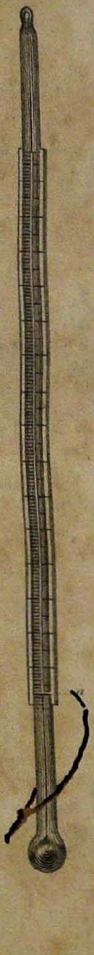


Fig. II.

PLATE XII.

TWO views of a thermometer which has the ſcale ſo conſtructed as to admit of its being introduced into any cavity that can receive the ball. The ſcale is moveable; but the freezing point is marked on the ſtem or glaſs.

FIGURE I. A front view, expoſing the glaſs ſtem of the thermometer, through which the diviſions marked upon the concave ſurface of the ſliding ivory ſcale which embraces it, are very diſtinctly ſeen.

a The freezing point, which is marked upon the ſtem by a ſcratch on the glaſs.

FIGURE II. A ſide view, ſhowing the degrees marked near the edge of the convex ſide of the ivory ſcale.

The thermometer is to be adjuſted for meaſuring high or low degrees of heat, by bringing any number marked upon the ſcale oppoſite the freezing point, and counting either upwards or downwards.

PROPOSALS FOR THE RECOVERY OF PERSONS APPARENTLY DROWNED.

HAVING been requeſted by a principal member of the ſociety eſtabliſhed for the recovery of perſons apparently drowned, to commit my thoughts on that ſubject to paper, I readily complied, hoping, that although I have had no opportunities of making actual experiments upon drowned perſons, it might be in my power to throw ſome lights on a ſubject ſo cloſely connected with the inquiries which, for many years, have been my buſineſs and favourite amuſement: I therefore collected together my obſervations and experiments relative to the loſs and recovery of the actions of life, which I now offer to the public. The endeavour to recover perſons apparently drowned, is a new practice, and has furniſhed, as yet, few important and clear facts: our knowledge of the animal œconomy is ſo imperfect, that I am afraid our reaſoning from that alone, muſt not be relied on, in a queſtion ſo intereſting to the cauſe of humanity. But let us reaſon, as well as we can, from the few data we have; and let every man bring forward, freely, the obſervations he has made, that the ſubject thus fairly before the public, may in time, by its united efforts, be more perfectly underſtood.

I ſhall conſider an animal, apparently drowned, as not dead; but that only a ſuſpenſion of the actions of life has taken place. The difference between a ſuſpenſion of the actions of life, and abſolute death, is well illuſtrated by the common ſnail when drowning. If a ſnail is immerſed in water and kept there, certain voluntary and inſtinctive actions take place; but after remaining a certain time covered by the water, all theſe actions ceaſe. Hence the animal being relaxed, naturally comes out of the ſhell in that ſtate; its ſtomach is filled with water, and the body appears larger than natural; but without motion. Theſe actions continue

thus suspended, till either the cause of suspension be removed, or some other stimulus shall bring the parts into action: but under such circumstances life cannot be preserved for any considerable length of time; and when the stimulus which precedes death takes place, the whole animal is thrown into action, and in that contracted state, possibly, absolute death is produced. A state of relaxation should therefore (where an universal violence has not been committed) be considered as the criterion of life; and even in such cases, should be for some time admitted as a probable reason for supposing life still to exist.

If an animal appears so far dead as to have lost all the actions characteristic of life, yet a certain degree of action in all the parts will be produced when absolute death is taking place; and that animal, being still susceptible of stimulus, is recoverable if the proper stimulus could be applied.

It is asserted that men have recovered the actions of life, even after the contraction, in consequence of the stimulus which precedes death, has taken place. If this be true, which I very much doubt, the stimulus must first produce relaxation, which is an action dependent on life.

This is probably the case in the first appearances of death from all violent accidents, except those caused by lightning, electricity, an universal shock, a blow on the stomach, a violent affection of the mind, or some other modes by which absolute death may be instantaneously produced; which all appear to act in the same way, producing absolute and instant death. For in cases which have fallen under my observation, the concomitant circumstances have resembled those which attend death caused by lightning or electricity; such as a total and instantaneous privation of sense and motion without convulsions; consequently, no rigor of muscles having been produced, and the blood remaining uncoagulated; differing entirely in these respects from what appears in persons deprived of sense and life by any injury done the brain. It seems only possible to account for this effect of a blow on the stomach, from the connection subsisting between that viscus and every part of the body, at least with vital parts; the blow, most probably, causing instant death in that organ, of which

the death of the whole animal is the conſequence*. When death takes place from violent affections of the mind, it muſt be referred to the univerſal influence which the mind has over the body.

To aſcertain when a body is deprived of life, it is firſt neceſſary to know in what manner apparent death took place; whether in the common way, or from the vital actions being too long ſuſpended. In either caſe, ſtiffneſs of the muſcles is probably the moſt certain and moſt evident proof of abſolute death; ſince that ariſes from the ſtimulus immediately preceding death having taken place. But if the privation of life is produced by any of the modes abovementioned, which kill inſtantaneouſly and univerſally, the ſtimulus which produces ſtiffneſs is not allowed time to act, and the muſcles are all left in a relaxed ſtate. Yet this ſtate of relaxation muſt not, on that account be always conſidered as a proof of life ſtill remaining.

A degree of flaxidity in the eye-balls, which produces glaſſineſs, is a certain mark of death; but is, however, only a ſecondary mode of aſcertaining it in thoſe inſtances where the body becomes ſtiff; but may be the firſt mode where abſolute death takes place inſtantaneouſly; and putrefaction will be the ſecond; while in the other caſes putrefaction will be the third.

That I may more fully explain my ideas upon this ſubject, it will be neceſſary to ſtate ſome propoſitions.

Firſt; that ſo long as the animal retains the ſuſceptibility of impreſſion, though deprived of the action of life, it will, moſt probably, retain the power of action when impreſſed; therefore the action may frequently be ſuſpended, and yet recoverable; but, when the ſuſceptibility of impreſ-

* I ſhould conſider the ſituation of a perſon drowned to be ſimilar to that of a perſon in a trance. In both, the action of life is ſuſpended, without the power being deſtroyed: but I am inclined to believe, that a greater proportion of perſons recover from trances than from drowing; becauſe a trance is the natural effect of a diſpoſition in the perſon to have the action of life ſuſpended for a time; but drowning being produced by violence, the ſuſpenſion will more frequently laſt for ever, unleſs the power of life is rouſed to action by ſome applications of art.

ſion is deſtroyed, the action ceaſes to be recoverable. Secondly; it is neceſſary to mention, that I conſider part of the living principle as inherent in the blood*. Thirdly; that the ſtomach ſympathizes with every part of an animal, and that every part ſympathizes with the ſtomach; therefore, whatever acts upon the ſtomach as a cordial, or rouſes its natural and healthy actions, and whatever affects it ſo as to produce debility, has an immediate effect upon every part of the body. The laſt propoſition I have to make is, that every part of the body ſympathizes with the mind; for whatever affects the mind, the body is affected in proportion. Theſe ſympathies are ſtrongeſt with the vital parts; but beſides theſe univerſal ſympathies between the ſtomach, the mind, and all parts of the body, there are peculiar ſympathies; of which the heart ſympathiſing immediately with the lungs, is an inſtance. If any thing is received into the lungs, which is a poiſon to animal life, ſuch as inflammable air, volatile vitriolic acid, and many other well known ſubſtances, the motion of the heart immediately ceaſes, even much ſooner than if the trachea had been tied; and, from experiments, it appears, that any thing ſalutary to life, applied to the lungs, will reſtore the heart's action after it has been at reſt ſome time.

I ſhall divide violent deaths into three kinds: Firſt, where a ſtop is only put to the action of life in the animal, but without any irreparable injury to a vital part; which action, if not reſtored in a certain time, will be irrecoverably loſt. The length of that time is ſubject to conſiderable variation, depending on circumſtances with which we are at preſent unacquainted. The ſecond is, where an injury is done to a vital part; as by taking away blood till the powers of action are loſt; or by a wound or preſſure being made on the brain or ſpinal marrow, while life remains

* That the living principle is inherent in the blood, is a doctrine which the nature of this account will not allow me to diſcuſs: thus much, however, it may be proper to ſay, that it is founded on the reſult of many obſervations and experiments. But it may be thought neceſſary I ſhould here give a definition of what I call the living principle: ſo far then as I have uſed the term, I mean to expreſs that principle which preſerves the body from diſſolution with or without action, and is the cauſe of all its actions.

in the ſolids ſufficient for the preſervation of the animal, if action could be reſtored to the vital parts. The third is, where abſolute death inſtantly takes place in every part, as is often the caſe in ſtrokes of lightning; in the common method of killing eels, by throwing them on ſome hard ſubſtance, in ſuch manner as that the whole length of the animal ſhall receive the ſhock at the ſame inſtant; by a blow on the ſtomach; by violent affections of the mind, and by many diſeaſes, in all which caſes the muſcles remain flexible*.

How far that may be ſtrictly conſidered as a violent death, which is cauſed by affections of the mind, I will not pretend to ſay; but if it is to have a place in that claſs, it muſt be ranked with thoſe which happen from lightning, and a blow on the ſtomach: and in moſt caſes of perſons drowned, I can eaſily conceive the mind to be ſo much affected, prior to the immerſion, and in the moment immediately ſucceeding it, as to make a material difference in the power of recovery. In many ſudden deaths, ariſing from violence, and even from diſeaſe, death ſhall take place ſo immediately, that the muſcles neither contract, nor does the blood coagulate.

The preſent conſideration is, under which of the kinds of violent death drowning can be claſſed or arranged? I am of opinion it will moſt commonly come under the firſt; and upon that ground I ſhall principally conſider the ſubject, always ſuppoſing the body to remain flaccid.

The loſs of motion in drowning ſeems to ariſe from the loſs of reſpiration, and the immediate effects which that has upon the other vital motions of the animal; except what may have ariſen from the affections of the mind. The privation of breathing appears, however, to be the firſt cauſe; and the heart's motion ceaſing, to be the ſecond or conſequent; therefore moſt probably the reſtoration of breathing is all that is neceſſary to reſtore the heart's motion: for if ſufficient life ſtill exiſts to produce

* On the other hand, when an eel is killed by chopping it into a number of pieces, the powers of life are by thoſe means rouſed into action; and, as every part dies in that active ſtate, every part is found ſtiff after death. This explains the cuſtom of cutting fiſh into pieces while yet alive, in order to make them hard, uſually known by the name of crimping.

that effect, we may ſuppoſe every part equally ready to move the very inſtant in which the action of the heart takes place, their actions depending ſo much upon it. What makes it very probable, that in recovering perſons drowned, the principal effect depends upon air being thrown into the lungs, is, what happens at the birth of children, when two much time has intervened between the interruption of that life which is peculiar to the fœtus, and that which depends on breathing; they then loſe altogether the diſpoſition for this new life; and in ſuch caſes, there being a total ſuſpenſion of the actions of life, the child remains to all appearance dead; and would certainly die, if air were not thrown into its lungs, and by ſuch means the firſt principle of action reſtored. To put this in a ſtill clearer light, I will give the reſult of ſome experiments which I made in the year 1755 upon a dog.

A pair of double bellows were provided, conſtructed in ſuch a manner as by one action to throw freſh air into the lungs, and by another to ſuck out again the air which had been thrown in by the former, without mixing them together. The muzzle of theſe bellows was fixed into the trachea of a dog, and by working them he was kept perfectly alive. While this artificial breathing was going on, I took off the ſternum of the dog, and expoſed the lungs and heart; the heart continued to act as before, only the frequency of its action was conſiderably increaſed. When I ſtopped the motion of the bellows, the heart became gradually weaker and leſs frequent in its contractions, till it entirely ceaſed to move. By renewing the action of the bellows, the heart again began to move, at firſt very faintly, and with long intermiſſions; but by continuing the artificial breathing, its motion became as frequent and as ſtrong as at firſt. This proceſs I repeated upon the ſame dog ten times, ſometimes ſtopping for five, eight, or ten minutes, and obſerved that every time I left off working the bellows, the heart became extremely turgid with blood, the blood in the left ſide becoming as dark as that in the right; which was not the caſe when the bellows were working. Theſe ſituations of the animal appeared to me exactly ſimilar to drowning.

Death in perſons drowned has been accounted for, by ſuppoſing that

the blood, rendered unfit for the purpoſes of life, by being deprived of the action of the air in reſpiration, is ſent in a vitiated ſtate to the brain and other vital parts; by which means the nerves loſe their effect upon the heart, and the heart in conſequence its motion. This, however, I am fully convinced is falſe; firſt, from the experiments on the dog, in which a large column of blood ſo vitiated (conſiſting of what had been propelled from the heart, after reſpiration ſtopped, and might be ſuppoſed the cauſe of the heart ceaſing to act, together with all that remained in the heart and pulmonary veins) was again puſhed forward without any ill effect having been produced: and next, from the return to life of perſons drowned and children ſtill-born, which, were ſuch a ſuppoſition true, could never happen; unleſs we imagine a change of the blood to take place in the brain, prior to the reſtoration of the heart's motion. This reſtoration muſt therefore depend immediately on the application of air to the lungs, and not on the effects which air has upon the blood, or that blood upon the vital parts.

If the affections of the mind have had any ſhare in the ceſſation of action in the heart, its motion will not be ſo eaſily reſtored as in other caſes. In our attempts to recover thoſe who have been drowned, it might therefore be proper to inquire, if there had been time ſufficient for the perſon to form any idea of his ſituation, previous to his being plunged into the water; as it is not unlikely, that the agitated ſtate of mind might aſſiſt in killing him; and in ſuch caſe I ſhould very much doubt the probability of reſtoring him to life. In the hiſtory of thoſe who have, and who have not been recovered, could the difference be aſcribed to any ſuch cauſe, it might lead to ſomething uſeful; as in thoſe who have had an intention to deſtroy themſelves, a great difference in the chance of recovery may ariſe, from the mind having been previouſly very much affected.

It frequently happens, in the caſe of drowning, that aſſiſtance cannot be procured till a conſiderable time after the accident; every moment of this delay renders recovery more precarious, the chances of which are not only diminiſhed in the parts where the firſt powers of action principally reſide, but alſo in every other part of the body.

In offering my ſentiments on the method of treating perſons who are apparently drowned, I ſhall ſay firſt, what I would recommend to have done; ſecondly, what I would wiſh might be avoided.

When aſſiſtance is called in, ſoon after the immerſion, perhaps blowing air into the lungs may be ſufficient to effect a recovery*; but if a conſiderable time, as an hour, has been loſt, it will ſeldom be ſufficient; the heart, in all probability, having by that time loſt its intimate connection with the lungs. It will in theſe caſes, therefore, be proper to apply, mixed with the air, ſuch ſtimulating medicines as the vapour of volatile alkali; which may eaſily be done, by holding ſpirits of hartſhorn in a cup under the receiver of the bellows. I would adviſe the air and volatile alkali to be thrown in by the noſe, rather than the mouth, as the laſt mode of adminiſtering, by producing ſickneſs, is more likely to depreſs than rouſe the living principle. It will be ſtill better if it can be done by both noſtrils, as applications of this kind to the olfactory nerves certainly rouſe the living principle and put the muſcles of reſpiration into action, and therefore are the more likely to excite the action of the heart: beſides, that affections of theſe nerves are known to act more immediately on the living principle; ſince while a ſtrong ſmell of very ſweet flowers, as orange flowers, will in many cauſe fainting, the application of vinegar will as immediately reſtore the powers to action again. All perfumes in which there is ſome acid, rather rouſe than depreſs, as the ſweet-brier, eſſence of lemon, &c. If during the operation of the bellows, the larynx be gently preſſed againſt the œſophagus and ſpine, it will prevent the ſtomach and inteſtines being too much diſtended by the air, and leave room for the application of more effectual ſtimuli to thoſe parts. This preſſure, however, muſt be conducted with judgment and caution, ſo that the trachea and the aperture into the larynx may both be left perfectly free. While this buſineſs is going on, an aſſiſtant ſhould prepare bed-cloaths, carefully brought to the proper degree of heat. I conſider

* Perhaps the dephlogiſticated air, deſcribed by Dr. Prieſtley, may prove more efficacious than common air. It is eaſily procured, and may be preſerved in bottles or bladders for that purpoſe.

heat as congenial with the living principle; increaſing the neceſſity of action it increaſes action: cold, on the other hand, leſſens the neceſſity, and of courſe the action is diminiſhed: to a due proportion of heat, therefore, the living principle owes its vigour; and, from obſervations and experiments, it appears to be a law of Nature, in animal bodies, that the degree of external heat ſhould bear a proportion to the quantity of life; when it is weakened, this proportion requires great accuracy in the adjuſtment; while greater powers of life allow a greater latitude*.

I was led to make theſe obſervations, by attending to perſons who are froſt-bitten; the effect of cold in ſuch caſes being that of leſſening the living principle. The powers of action remain as perfect as ever, but weakened; and heat is the only thing wanting to put theſe powers into action: yet that heat muſt at firſt be gradually applied, and proportioned to the quantity of the living principle; which increaſing, the degree of heat may likewiſe be increaſed. If this method is not obſerved, and too great a degree of heat is at firſt applied, the perſon or part loſes entirely the living principle, and mortification enſues. Such a proceſs invariably takes place with regard to men; and the ſame thing, I am convinced, happens to other animals. For if an eel is expoſed to a degree of cold ſufficiently intenſe to benumb it till the remains of life are ſcarcely perceptible, and ſtill retained in a cold of about 40°, this ſmall proportion of living principle will continue for a conſiderable time without diminution or increaſe; but if the animal is afterwards placed in a heat about 60°, after ſhowing ſtrong ſigns of returning life, it will die in a few minutes. Nor is this circumſtance peculiar to the diminution of life by cold. The ſame phænomena take place in animals who have been very much reduced by hunger.

If a lizard, or ſnake, when it goes to its autumnal hiding place, is not ſufficiently fat, the living powers are, before the ſeaſon permits it to come out, very conſiderably weakened; perhaps ſo much as not to admit of

* It is upon theſe principles that cold air is found of ſo much ſervice to people who are reduced by diſeaſe, as the confluent ſmallpox, and fevers, by diminiſhing heat in proportion to the diminution of life; or leſſening the neceſſity of the body's producing its own cold.

the animal being again reſtored. If animals, in a torpid ſtate, are expoſed to the ſun's rays, or placed in any ſituation which by its warmth would give vigour to thoſe of the ſame kind, poſſeſſed of a larger ſhare of life, they will immediately ſhow ſigns of increaſed life, but quickly ſink under the experiment and die; while others, reduced to the ſame degree of weakneſs, as far as appearances can diſcover, will live for many weeks, if kept in a degree of cold proportioned to the quantity of life they poſſeſs.

I obſerved, many years ago, in ſome of the colder parts of this iſland, that when intenſe cold had forced blackbirds or thruſhes to take ſhelter in out-houſes, ſuch of them as had been caught, and were, from an ill-judged compaſſion, expoſed to a conſiderable degree of warmth, died very ſoon. The reaſon of this I did not then underſtand; but I am now ſatisfied that it was owing, as in other inſtances, to the degree of heat being increaſed too ſuddenly for the proportion of life remaining in the animal.

From theſe facts it appears, that warmth cauſes a greater exertion of the living powers than cold; and that an animal in a weakly ſtate may be obliged by it to exert a quantity of the action of life ſufficient to deſtroy the very powers themſelves*. The ſame effects probably take place even in perfect health; it appearing, from experiments made in a heated room, that a perſon in health, expoſed to a great degree of heat, found the actions of life accelerated ſo much, as to produce at laſt faintneſs and debility†.

If bed-cloaths are put over the drowned perſon, ſo as ſcarcely to touch him, ſteam of volatile alkali, or of warm balſams and eſſential oils, may be ſo conveyed as to come in contact with many parts of his body; and it will certainly prove advantageous, if the ſame kind of ſteams can be conveyed into the ſtomach, as that ſeat of univerſal ſympathy will be rouſed by ſuch means. This may be done by a hollow bougie and a ſyringe; but the operation ſhould be performed with all poſſible expedition, becauſe the inſtrument, by continuing in the mouth may produce ſickneſs,

* It is upon this principle that parts mortify in conſequence of inflammation.

† Vide Phil. Tranſ. for the year 1775, vol. 65. p. 111.

an effect I ſhould chuſe to avoid, unleſs it is intended to produce the action of vomiting. Some of the ſtimulating ſubſtances, which are of a warm nature, and have an immediate effect, as ſpirits of hartſhorn, peppermint-water, juice of horſe-raddiſh, and many others which produce a more laſting ſtimulus in a fluid ſtate, and are found to quicken the pulſe of a man in health, as balſams and turpentines, may be thrown into the ſtomach; but the quantity muſt be ſmall, as they have a tendency to produce ſickneſs; for it may be imagined, that what would produce debility, or leſſen action when in health, would, in oppoſite circumſtances prevent actions from taking place. The application of ſteams, and other ſubſtances, ſhould alſo be thrown up by the anus; and the proceſs recommended under the firſt head of treatment ſhould ſtill be continued, while that recommended under the ſecond is putting in practice, the laſt being only an auxiliary to the firſt. The firſt, in many caſes, may ſucceed alone; but the ſecond without the firſt, muſt, I think, always fail where the powers of life are conſiderably weakened. Motion may poſſibly be of ſervice; it may at leaſt be tried; but, as it has leſs effect than any other of the uſually preſcribed ſtimuli, it ſhould be the laſt applied*. I would recommend to the operator the ſame care in regulating the application of every one of theſe methods, as I did before in that of heat; as each may have the ſame property of entirely deſtroying the feeble action which they have excited, if adminiſtered in too great a proportion: inſtead therefore of increaſing and haſtening the operations, on the firſt ſigns of returning life being obſerved, as is uſually done, I ſhould wiſh them to be applied more gently and gradually, that their increaſe afterwards may be directed, as nearly as poſſible, in a degree proportioned to the powers as they ariſe. As the heart is commonly the laſt part that

* Electricity has been known to be of ſervice, and ſhould be tried when other methods have failed. It is probable, the only method we have of immediately ſtimulating the heart; all other methods being more by ſympathy. I have not mentioned injecting ſtimulating ſubſtances directly into the veins, though it might be ſuppoſed a proper expedient; becauſe, in looking over my experiments on that ſubject, I found none where animal life received increaſe by that method.

ceaſes to act, it is probably the firſt part that takes on the action of recovery. When it begins to move, I would adviſe leſſening the application of air to the lungs, and enjoin thoſe employed to obſerve with great attention when the muſcles of reſpiration begin to act, that our endeavours may not interfere with their natural exertions; yet that we may be ſtill ready to aſſiſt. I would by all means diſcourage blood-letting; which I think weakens the animal principle, and life itſelf; conſequently leſſens both the powers and diſpoſitions to action; and I would adviſe being careful not to call forth any diſpoſition that might depreſs, by introducing things into the ſtomach, which ordinarily create nauſea; as that alſo will have a ſimilar effect, except it can be carried ſo far as to excite the action of vomiting, by which the ſtomach could relieve itſelf. It will be prudent, likewiſe, to avoid adminiſtering by the anus, any thing that may be likely to produce an evacuation that way; every ſuch evacuation tending to leſſen the animal powers. I have purpoſely avoided ſpeaking of the fumes of tobacco, which always produce ſickneſs or purging, according as they are applied.

Whoever is appointed for the purpoſe of recovering drowned perſons, ſhould have an aſſiſtant, well acquainted with the methods intended to be made uſe of; that while the one is going on with the firſt and moſt ſimple methods, the other may be preparing what elſe may be proper, ſo that no time may be loſt between the operations; and this is the more neceſſary, as the firſt means recommended, will, in all caſes, aſſiſt the ſecond; and both together may often be attended with ſucceſs, though each ſeparately might have failed.

A proper apparatus is alſo eſſentially neceſſary to the inſtitution; a deſcription of which I here annex. Firſt, a pair of bellows, ſo contrived, with two ſeparate cavities, that by expanding them, when applied to the noſtrils or mouth of a patient, one cavity may be filled with the common air, and the other with air ſucked out from the lungs; and by ſhutting them again, the common air may be thrown into the lungs, and that which is ſucked out of the lungs be diſcharged into the room. The pipe of theſe ſhould be flexible, in length a foot or a foot and a half,

and at leaſt three-eights of an inch in width: as the artificial breathing may be continued by ſuch means, while the other operations, except the application of the ſtimuli to the ſtomach, are going on; which cannot conveniently be done if the nozzle of the bellows be introduced into the noſe. The end next the noſe ſhould be double, and applied to both noſtrils. Secondly, a ſyringe, with a hollow bougie, or flexible catheter, of ſufficient length to go into the ſtomach, and convey any ſtimulating matter into it, without affecting the lungs. Thirdly, a ſmall pair of bellows, ſuch as are commonly uſed in throwing fumes of tobacco up the anus, by which ſtimulating fluids, or even fumes may be thrown in.

I ſhall conclude this account by propoſing, that all who are employed in this practice be particularly required to keep an accurate journal of the means uſed, and the degree of ſucceſs attending them; whence we may be furniſhed with facts ſufficient to enable us to draw concluſions, on which a certain practice may hereafter be eſtabliſhed.

OBSERVATIONS TENDING TO SHEW THAT THE WOLF, JACKAL, AND DOG, ARE ALL OF THE SAME SPECIES.

THE true diſtinction between different ſpecies of animals muſt ultimately, as appears to me, be gathered from their incapacity of propagating with each other an offspring capable again of continuing itſelf by ſubſequent propagations: thus the horſe and aſs beget a mule capable of copulation, but incapable of begetting or producing offspring. If it be true, that the mule has been known to breed, which muſt be allowed to be an extraordinary fact, it will by no means be ſufficient to determine the horſe and aſs to be of the ſame ſpecies; indeed, from the copulation of mules being very frequent, and the circumſtance of their breeding very rare, I ſhould rather attribute it to a degree of monſtroſity in the organs of the mule which conceived, as not being a mixture of two different ſpecies, but merely thoſe of either the mare or female aſs. This is not ſo far-fetched an idea, when we conſider that ſome true ſpecies produce monſters, which are a mixture of both ſexes, and that many animals of diſtinct ſex are incapable of breeding at all. If then we find Nature in its moſt perfect ſtate deviating from general principles, why may not it happen likewiſe in the production of mules; ſo that ſometimes a mule ſhall breed from the circumſtance of its being a monſter reſpecting mules?

The time of uterine geſtation being the ſame in all the varieties of every ſpecies of animals, it becomes a neceſſary circumſtance towards determining a ſpecies.

The affinity between the fox, wolf, jackal, and ſeveral varieties of the dog, in their external form and ſeveral of their properties, is ſo ſtriking, that they appear to be only varieties of the ſame ſpecies. The fox would ſeem to be farther removed from the dog than either the jackal or wolf,

at leaſt in diſpoſition, being naturally a ſolitary animal, and neither ſo ſociable reſpecting its own ſpecies or man: from which I ſhould infer that it is only allied to the dog by being of the ſame genus. It is confidently aſſerted by many, that the fox breeds with the dog; but this has not been accurately aſcertained: if it had, the enquiry would probably have been carried further; and once breeding, according to what we have ſaid, does not conſtitute a ſpecies; this, however, is a part I mean to inveſtigate. I do not know if, in a wild ſtate, there ever is in the ſame country a variety in any ſpecies of animal; but am inclined to think there never is: if ſo, as both wolves and foxes inhabited this country, they cannot then be of the ſame ſpecies.

Wolves, as alſo jackals, are found in herds; and the jackal is ſo little afraid of the human ſpecies, that, like a dog, it comes into houſes in ſearch of food, more like a variety of the dog, the conſequence of cultivation rather than of chance. It would appear to be much the moſt familiar of the two; for we ſhall find, that in its readineſs to copulate with the dog, and its familiarity with the dog afterwards, it is ſomewhat different from the wolf; however, this may depend on accident. The wolf being an animal well known in Europe, the part of the world where natural hiſtory is particularly cultivated, ſome pains have been taken to aſcertain, whether or not it was of the ſame ſpecies with the dog; but, I believe, it has been hitherto conſidered as only belonging to the ſame genus.

Accident often does as much for natural hiſtory as premeditated plans, eſpecially when Nature is left to itſelf. The firſt inſtance of the dog and wolf breeding in this country ſeems to have been about the year 1766. A Pomeranian bitch of Mr. Brookes's, in the New Road, was lined only once by a wolf, and brought forth a litter of nine healthy puppies. The veracity of Mr. Brookes is not to be doubted, reſpecting the bitch having been lined by a wolf; yet, as it was poſſible ſhe might have been lined by ſome common dog without his knowledge, the fact was not, in that, clearly made out; but it has been ſince aſcertained, that the dog and wolf will breed. One of the above-mentioned litter was preſented to me by

Mr. Brookes, who likewiſe informed me that others had been purchaſed by different noblemen and gentlemen, and named Lord Clanbraſſil as having bought a bitch-puppy. I reſerved mine for the purpoſe of experiment; and from obſervation, it appeared that its actions were not truly thoſe of a dog; having more quickneſs of attention to what paſſed; being more eaſily ſtartled, as if particularly apprehenſive of danger; quicker in tranſitions from one action to another; being not ſo ready to the call; and leſs docile. From theſe peculiarities it loſt its life, having been ſtoned to death in the ſtreets for a mad dog.

Hearing that Lord Clanbraſſil's bitch had bred, Sir Joſeph Banks was ſo obliging as, at my requeſt, to write to his Lordſhip, who ſent the following account.

"SIR,

"About ſeventeen or eighteen years ago, the late Lord Monthermer and I happened to ſee a dog-wolf at Mr. Brookes's, who deals in animals, and lives in the New Road. The animal was remarkably tame; and it ſtruck us, for that reaſon, that a breed might be procured between him and a bitch.

"We promiſed Mr. Brookes a good price for puppies, if he ſucceeded. In about a year a bitch produced nine, and Lord Monthermer bought one; and I had another, which was a bitch. Lord Monthermer's died of fits in about two years: mine lived longer, and had puppies only once. One I gave to Lord Pembroke; but what became of it I do not remember. It was grand-daughter of the wolf by the dam, and got by a large pointer of mine.

"It might be conſidered, that Mr. Brookes's word was not ſufficient proof that the puppies were really got by the wolf; but the appearance of the animals, ſo totally different from all others of the canine ſpecies, did not leave a doubt upon our minds; and I remember Hans Stanley, who had adopted Buffon's opinion, was thoroughly convinced upon ſeeing mine. The animals had the ſhape of the wolf refined; the fur long, but almoſt as fine as that of the black fox.

"I am afraid I have treſpaſſed too much upon your time, and will only beg you will be aſſured nothing can give me more pleaſure than any opportunity of aſſuring you how truly

I am, Sir, &c.

Jan. 7, 1787.

CLANBRASSIL."

Upon the ſuppoſition that Mr. Brookes's bitch was not lined by a dog, but by the wolf, which I think we have no reaſon to doubt, the ſpecies of the wolf is aſcertained; but chooſing to trace this matter ſtill further, and hearing that Lord Pembroke's bitch had likewiſe bred, I was deſirous to know the truth of it; and as his lordſhip was in France, I took the liberty of writing to Lord Herbert, and received the following anſwer.

"SIR, WILTON-HOUSE, DEC. 20, 1786.

"The half-bred wolf-bitch you allude to was given, as I always underſtood, to Lord Pembroke by Lord Clanbraſſil. She might, perhaps, have been bought at Brookes's by him. She had four litters, one of ten puppies, by a dog between a maſtiff and a bull-dog. One of theſe was given to Dr. Eyre, at Wells in Somerſetſhire, and one to Mr. Buckett, at Stockbridge. The ſecond litter was of nine puppies, ſome of which were ſent to Ireland, but to whom I know not. This litter was by a different dog, but of the ſame breed as the firſt. The third litter was of eight puppies, by a large maſtiff. Two of theſe were, I believe, ſent to the preſent Duke of Queenſberry. The fourth litter conſiſted of ſeven puppies; two of which were ſent to M. Cerjat, a gentleman who now reſides at Lauſanne in Switzerland, and is famous for breaking dogs remarkably well. Theſe two puppies were, however, naturally ſo wild and unruly, that he found it impoſſible to break them. She died four years ago, and the following inſcription was put over the place where ſhe is buried in this garden, by Lord Pembroke's orders.

Here lies Lupa,
whose grandmother was a wolf,
whose father and grand-father were dogs, and whose
mother was half wolf and half dog. She died
on the 16th of October, 1782,
aged 12 years.

"I am sorry it is not in my power to give you any better account; but if you think proper to write to Lord Pembroke, who is at Paris, I am convinced he will be very happy to give you any further information.

I am, &c.

HERBERT."

Buffon, whose remarks in natural history are well known, made experiments to ascertain how far the wolf and dog were of the same species, but without success. He says, "A she-wolf, which I kept three years, altho' shut up very young, and along with a greyhound of the same age, in a spacious yard, could not be brought to agree with it, nor endure it, even when she was in heat. She was the weakest, yet the most mischievous; provoking, attacking, and biting the dog, which at first only defended itself, but at last killed her." And in another part of his work, he makes the following observation: "The dog, the wolf, the fox, and the jackal, form a genus, of which the different species are really so nearly allied to each other, and of which the individuals resemble each other so much, particularly by the internal structure and parts of generation, that it is difficult to conceive why they do not breed together*."

* In the Supplement to his Works, he gives the following account which had been sent to him. "A very young she-wolf, brought up at the Marquis of Spontin's, at Namur, had a dog, of nearly the same age, kept with it as a companion. For two years they were at liberty, coming and going about the apartments, the kitchen, the stables, &c. lying under the table, and upon the feet of those who sat round it. They lived in the greatest familiarity.

"The dog was a strong greyhound. The wolf was fed on milk for six months; after that, raw meat was given her, which she preferred to that which was dressed. When she ate no

This part of natural hiſtory lay dormant till Mr. Gough, who ſells birds and has a collection of animals on Holborn-Hill, repeated the experiment on a wolf-bitch, which was very tame, and had all the actions of a dog under confinement. A dog is the moſt proper ſubject for comparison, as we have opportunities of being acquainted with its diſpoſition and mode of expreſſing its ſenſations, which are moſt diſtinguiſhable in the motion of the ears and tail; ſuch as pricking up the ears when anxious, wiſhing, or in expectation; depreſſing them when ſupplicant, or in fear; raiſing the tail in anger or love, depreſſing it in fear, and moving it laterally in friendſhip; and likewiſe in raiſing the hair on the back from many affections of the mind. This animal became in heat in the month of December 1785; and Mr. Gough having an idea of obtaining a breed from wild animals, as monkies, leopards, &c. he was deſirous to have the wolf lined by ſome dog; but ſhe would not allow any dog to come near her; probably from being always chained, and not accuſtomed to be with dogs. She was held, however, while a greyhound dog lined her, and they faſtened together exactly like the dog and bitch. While in conjunction ſhe remained pretty quiet; but when at liberty, endeavoured to fly at the dog; yet in this way was twice lined. She conceived, and brought forth four young ones; and though the time ſhe went with young was not exactly known, it was believed to be the ſame as in the bitch. Two

no one durſt approach her; but at other times people might do as they pleaſed, provided they did not uſe her ill. At firſt ſhe made much of all the dogs which were brought to her; but afterwards ſhe gave the preference to her old companion, and from that time ſhe became very fierce if any ſtrange dog approached her. She was lined for the firſt time on the 25th of March; this was frequently repeated while her heat continued, which was ſixteen days; and ſhe littered the 6th of June, at eight o'clock in the morning; the period of geſtation was therefore ſeventy-three days at the moſt*. She brought forth four young ones of a blackiſh colour, ſome of whoſe feet, and a part of the breaſt, were white; in this reſpect taking after the dog, who was black and white. From the time ſhe littered ſhe became ſurly, and ſet up her back at thoſe who came near her; did not know her maſters, and would even have killed the dog, if it had been in her power."

* This is a longer period than in the bitch by at leaſt ten days; but as the account was made from the firſt time of her being lined, and ſhe was in heat for a fortnight, and lined in that time, it is very probable, if the time was known when ſhe conceived, that it would prove to be the ſame period as in the dog.

of theſe puppies were like the dog in colour, who had large black ſpots on a white ground; another was of a black colour; the fourth of a kind of dun, and would probably have been like the mother. She took great care of them, yet did not ſeem very anxious when one was taken from her by the keeper; nor did ſhe ſeem afraid when ſtrangers came into the room. Unfortunately theſe experiments were carried no further; one of the puppies being ſold to a gentleman, who carried it to the Eaſt-Indies; and the other three, one of which I was to have had, were killed by a leopard. The ſame wolf was in heat in December 1786, and was lined ſeveral times by a dog. She pupped on the 24th of February 1787, and had ſix puppies, one of which, a bitch, I had, and kept it till it was in heat; but miſſed the opportunity of having her lined; that loſs, however, was made up by a wolf-bitch belonging to James Symmons, Eſq. of Groſvenor-Houſe, Milbank; the hiſtory of which is as follows.

This female-wolf had been in his poſſeſſion ſome time, had been lined by a dog, and brought forth ſeveral puppies, which I ſaw in company with Sir Joſeph Banks, ſoon after Mr. Gough's wolf, the ſubject of my former Paper, had produced her litter; ſo that theſe puppies were nearly of the ſame age with mine. Mr. Symmons reared them all; but one only was a female, which more reſembled the mother or wolf-kind, than any of the others. I communicated to Mr. Symmons my wiſh, that we ſhould endeavour to prove the fact of the wolf and dog being of the ſame ſpecies, by having either his female or mine lined by a dog. This he very readily acceded to; and his bitch received the dog on the 16th, 17th, and 18th of December 1788; and the 18th of February following, ſhe brought forth eight puppies, all of which ſhe reared.

If we reckon from the 16th of December, ſhe went ſixty-four days; but if we reckon from the 17th, the mean time, then it is ſixty-three days, the uſual time for a bitch to go with pup. Theſe puppies are the ſecond remove from the wolf and dog, and ſimilar to that given by my Lord Clanbraſſil to the Earl of Pembroke, which likewiſe bred again. (See Philoſophical Tranſactions, vol. LXXVII. p. 255.) It would have equally proved the ſame fact if ſhe had been lined either by a wolf, a dog, or one of the males of her own litter.

It is remarkable, that there ſeems to be only one time in the year in which impregnation is natural to the wolf, which is the month of December: for Mr. Gough's wolf has always been in heat in that month; ſo was that of Mr. Symmons. The time of heat in his of the half-breed (which is nearly of the ſame age with mine) correſponded likewiſe with that of the mother, and of thoſe bred from Mr. Gough's wolf.

OF THE JACKAL.

THIS animal being ſo nearly allied to the dog, and only found wild like the wolf, I became deſirous of aſcertaining of what particular ſpecies it was; and while purſuing the ſubject, I was informed that Captain Mears, of the Royal Biſhop Eaſt-India-man, had brought home a bitch-jackal with young, which brought forth ſoon after his arrival; and that he had given the bitch-jackal and one puppy to Mr. Bailey, bird-merchant, in Piccadilly. I went to ſee them, and purchaſed the puppy, the ſubject of the following experiment, which we found to have diſpoſitions very ſimilar to thoſe of the half-bred wolf, beforementioned, which I had from Mr. Brookes.

To have a true hiſtory of this animal, I took the liberty of writing to Mr. Mears, who politely called upon me, and, at my requeſt, ſent me the particulars in a letter, of which the following is a copy.

"SIR,

"I had the honour of yours the 15th inſtant; and with regard to the female-jackal, I can aſſure you, that ſhe took a ſmall ſpaniel dog of mine on board my ſhip, the Royal Biſhop. I had her, when a cub, at Bombay; and a very ſhort time before I arrived in England ſhe got to heat, and enticed this ſmall dog into the long-boat, where I ſaw them repeatedly faſt together. I brought her to my houſe in the country, where ſhe pupped ſix puppies, one of which you have ſeen. Mr. Plaw, at No. 90, Tottenham-Court-Road, has a dog-puppy, which will be at your ſervice

at any time you chuſe to ſend for him, to make further experiments: I called on Mr. Plaw, and got his promiſe to let you have the dog.

I have the honour to be, Sir, &c.

Wm. Mears.

N° 107, Hatton-ſtreet,
16th Jan. 1786.

P.S. I had the bitch on board fourteen months."

Having taken this puppy into the country, and chained it up near a maſtiff-dog, they became very familiar, and ſeemingly fond of each other. When the bitch became firſt in heat, I could not get a proper dog; but about the latter end of September, ſhe being again in the ſame ſtate, ſeveral dogs were procured, and left with her. They appeared indifferent about her, probably from being in a ſtrange place; nor did ſhe ſeem inclined to be familiar with them. One was a large dog, which might not perhaps be able to line her; but ſhe was twice tied by a terrier, on the third of October. In a few weeks ſhe was evidently bigger; and on the 30th of November, in all fifty-nine days, brought forth five puppies. A few days before this period, ſhe dug a hole in the ground, by the ſide of her kennel, in which ſhe littered; and it was ſome time before ſhe would allow the puppies to ſtay in the kennel when put there. Some of theſe began to open their eyelids in about eight, others of them in nine days.

Here then being an abſolute proof of the jackal being a dog; and the wolf being equally made out to be of the ſame ſpecies; it now therefore becomes a queſtion, whether the wolf is from the jackal, or the jackal from the wolf, (ſuppoſing them but one origin)? From the ſuppoſition, that varieties become more tame in their nature than the originals, we ſhould be led to believe the wolf to be the original, and that the jackal was a ſtep towards civiliſation in that ſpecies of animal, and that therefore the jackal ſhould be conſidered as a variety of the wolf. There are wolves of various kinds, each country having a kind peculiar to itſelf; but the jackals that I have ſeen have been more uniform in reſemblance

to each other; probably becauſe only to be found in one country, the Eaſt-Indies. I am informed, however, that they vary in ſize. Whether the wolves of different countries are of one ſpecies, or ſome of them only of the ſame genus, I do not know; but I ſhould rather ſuppoſe them to be all of one ſpecies. An argument with me in favour of this ſuppoſition is, that, if there were wolves of diſtinct ſpecies, we ſhould have had by this time a great variety of every ſpecies of wolf, with the various diſpoſitions ariſing from variation in other reſpects; and thoſe varieties would now have been turned to very uſeful purpoſes, as in the caſe of the dog; for all the wolves we are yet acquainted with, ſhould have naturally the principle of cultivation in them, (as much probably as any animal) as much at leaſt as thoſe wolves we now know by the name of dogs. The not having a civiliſed ſpecies with all the characteriſtics of the wolf is, indeed, with me a proof that they are all of the ſame ſpecies with the dog. If they are all of the ſame ſpecies with the dog, then the firſt variety that took place would be ſtill in the character of a wolf, differing only in colour, or ſome trivial circumſtance, which could only ariſe from a difference in climate. The wolf is naturally, I believe, the inhabitant of cold climates, and little variety could take place while it remained in ſuch a ſituation: but if the jackal was originally a wolf, which had ſtrayed by accident more to the ſouthward, a greater variation from the genuine character might be produced, the difference of climate, and perhaps of food, becoming cauſes of variety. By continuing to inhabit a warm climate, this circumſtance would in time loſe part of its influence on the animal, and the jackal would admit of little more variety. This, however, is a point not now to be determined, it being difficult (perhaps impoſſible) to ſay, where the wolf became jackal, or (what we call) dog; and, as dogs differ much from one another, what particular dog may be conſidered as the firſt remove; or whether the jackal is the intermediate link connecting the wolf and dog. In any caſe, we may reckon three great varieties in this ſpecies, wolf, jackal, and dog; which again branch into their reſpective leſs obvious varieties. If the dog proves to be the wolf tamed, the jackal may probably be the dog returned to its wild ſtate; which leads

to another curious queſtion: whether, as animals vary from climate, cultivation, or what may be called differences in mode of living, they would return to their genuine character if allowed to go wild again in the original country?

To aſcertain the original animal of a ſpecies, all the varieties of that ſpecies ſhould be examined, to ſee how far they have the character of the genus, and what reſemblance they bear to the other ſpecies of the genus; for it is natural to ſuppoſe, that the original animal, or that which is neareſt to it, will have more of the true character of the genus, and a ſtronger reſemblance to the ſpecies neareſt allied to it, than any of the other varieties of its own ſpecies.

If we apply this to the dog, and conſider the fox as a diſtinct ſpecies, which there is great reaſon to believe it is, that variety which has the greateſt reſemblance to the fox, is to be looked upon as the original of all the others; which will prove to be the wolf.

Another mode of conſidering this ſubject, which is however ſecondary to the above, is by ſuppoſing that all animals were at firſt wild; and, therefore, that thoſe animals which remain wild, are the original ſtock; and that when we find animals far removed from their originals in appearance, the variation takes place in conſequence of cultivation; yet ſo that we can ſtill trace the gradation. What gives ſome force to this idea is, that where the dogs have been leaſt cultivated, there they ſtill retain moſt of their original character, or ſimilarity to the wolf or the jackal, both in ſhape and diſpoſition. The ſhepherd's dog, all over the world, has ſtrongly the character of the wolf or jackal; ſo that but little difference is to be obſerved, except in ſize and hair. That of ſize may perhaps take place under a variety of circumſtances; but difference in hair is, in general, although not always, influenced by climate. Thus the wolf has longer and ſofter hair than the jackal, becauſe a more northern animal; while the jackal of the Eaſt, and ſhepherd's dog in Portugal and Spain, have ſhorter and ſtronger hair than thoſe of Germany or Kamſchatka, from inhabiting warmer climates. But when we conſider their general ſhape, the character of countenance, the quick manner with the pricked and erect

ears, we muſt ſuppoſe them varieties of the ſame ſpecies. The ſmelling at the tale has been mentioned as characteriſtic of the dog; but, I believe, it is common to moſt animals, and only marks the male; for it is the moſt certain way the male has of knowing the female; and by another ſcent diſcovering whether the female is diſpoſed to receive the male, which is perhaps the final intention.

The Eſquimaux dog, and that found among the Indians as far ſouth as the Cherokees; the ſhepherd's dog in Germany, called Pomeranian; the ſhepherd's dog in Portugal and Spain; have all a ſtrong ſimilarity to the wolf and jackal.

Buffon, on the origin of dogs, ſeems to have had nearly the ſame idea; for he ſays the ſhepherd's dog is the original ſtock from which the different kinds of dog have ſprung.

As the wolf turns out to be a dog, it ſeems aſtoniſhing, that there was no account of dogs being found in America. But this I conſider as a defect in the firſt hiſtory of that country, as there are wolves; and I muſt think, in ſpite of all that has been ſaid to the contrary, that the Eſquimaux and Indian dog is only a variety from a wolf of that country, which had been tamed. Mr. Cameron, of Titchfield-ſtreet, who was many years among the Cherokees, and conſiderably to the weſtward of that country, obſerves, that the dog found there much reſembles the wolf; and that the natives conſider it to be a ſpecies of tame wolf; but as we come more among the Europeans who have ſettled there, the dogs are more of a mixed breed. Why the Cherokees ſhould have had only this kind of dog tranſported among them, while every other part of America has the varieties of Europe, is not eaſily ſolved.

The voice of animals is commonly characteriſtic of the ſpecies; but I ſhould ſuppoſe it to be only characteriſtic of the original ſpecies, and not always of the variety, and the ſuppoſition holds good in the dog ſpecies. Dogs may be ſaid to have a natural voice, and a variation, ariſing either from a variety having taken place in the ſpecies, or a kind of imitation. It would appear, that the voice of the wolf and the jackal is very ſimilar, being the natural voice, and is principally conveyed through the noſe, and

exactly resembling that noise in dogs, which is a mark of longing or melancholy, and also of fondness; but having no resemblance to the bark of the dog, which they do not perform. However, in catching a jackal, when the animal found it could not escape, it yelped like the dog, which is a kind of barking, and which is, probably, the natural sound. Barking is peculiar to certain varieties of the dog kind, and even of those that bark, some do it less than others. The dogs in the South-sea islands do not bark: our greyhound barks but little; while the mastiff, and many of the smaller tribe, as spaniels, are particularly noisy in this way. There is reason to believe, that the frequency of this noise may arise from imitation; for the dogs in the South-sea islands learn to bark; the half-bred jackal barked; and so did the half-bred wolf, although but little; and others, as the hound, have a peculiar howl, by huntsmen, called the tongue; which noise, and the barking also, are both made by opening the mouth. A variety in the voice, or some parts of the voice, in varieties of the same species, is not peculiar to the dog.

It is a curious circumstance that variety not only takes place in colour and form, from the change of habits in the parents; but that the dispositions are also changed; and that the dispositions are most commonly changed in such a way as appears best adapted to the form of the animal. The change in the habits of the parent-animal, arise principally from its connection with the human kind, which has now succeeded in training dogs, so as to fit them, both in body and mind, for almost every purpose of human œconomy; as if man himself had formed them expressly with such intention; while, at the same time, he can only be considered as an occasional cause. For we may observe, that all the males of the wolf kind are nearly the same, and so are likewise those of the jackal; having little or no variety in their dispositions. Those of the half-breed, and even those that are three removes, although tame, yet they have not the docility of dogs, nor are they so immediately at the command of the human will; neither are they perfectly satisfied with an artificial life, having, when left to themselves, a propensity to fall back into their original instinctive principles.

The following account from Mr. Jenner of Berkley, to whom I gave a ſecond remove, viz. three parts dog, is very deſcriptive of this propenſity. "The little jackal-bitch you gave me is grown a fine handſome animal; but ſhe certainly does not poſſeſs the underſtanding of common dogs. She is eaſily loſt when I take her out, and is quite inattentive to a whiſtle. She is more ſhy than a dog, and ſtarts frequently when a quick motion is made before her. Of her inches ſhe is uncommonly fleet, much more ſo than any dog I ever ſaw. She can turn a rabbit in the field; ſhe is fond of ſtealing away and lying about the adjacent meadows, where her favourite amuſement is hunting the field-mouſe, which ſhe catches in a particular manner."

As animals are known to produce young which are different from themſelves in colour, form, and diſpoſitions, ariſing from what may be called, the unnatural mode of life, it ſhews this curious power of accommodation in the animal œconomy, that although education can produce no change in the colour, form, or diſpoſition of the animal; yet it is capable of producing a principle which becomes ſo natural to the animal, that it ſhall beget young different in colour and form; and ſo altered in diſpoſition, as to be more eaſily trained up to the offices in which they have been uſually employed; and having theſe diſpoſitions ſuitable to ſuch change of form.

It alſo becomes a queſtion, whether they would not go back again to their original ſtate, if put into the ſituation of the original from whence they ſprang; or acquire a form reſembling the original of that country where they are placed? I do not conceive that they muſt neceſſarily go back through the ſame changes; but I have ſome reaſon to ſuppoſe they would gradually return to a reſemblance of that original. And it would be difficult to prove, whether in many of the gradations they are progreſſive or retrograde. But this is a ſubject that requires particular attention and inveſtigation, and upon which I hope, ſome time or other, to be able to throw more light.

AN EXPERIMENT TO DETERMINE THE EFFECT OF EXTIRPATING ONE OVARIUM UPON THE NUMBER OF YOUNG PRODUCED.

IN all animals of diſtinct ſex, the females, thoſe of the bird kind excepted, have, I believe, two ovaria, and of courſe the oviducts are in pairs.

By diſtinct ſex I mean when the parts deſtined to the purpoſes of generation are of two kinds, each kind appropriated to an individual of each ſpecies, diſtinguiſhed by the appellation of male and female, and equally neceſſary to the propagation of the animal; the teſticles, with their appendages, conſtitute the male; the ovaria, and their appendages, the female ſex.

As the ovaria are the organs which, on the part of the female, furniſh what is neceſſary towards the production of the third, or young animal; and as females appear to have a limited portion of the middle ſtage of life allotted for that purpoſe; it becomes a queſtion, whether thoſe organs are worn out by repeated acts of propagation; or whether there is not a natural and conſtitutional period to that power on their part, even if ſuch power has never been exerted? If we conſider this ſubject in every view, taking the human ſpecies as an example, we ſhall diſcover that circumſtances, either local or conſtitutional, may be capable of extinguiſhing in the female the faculty of propagation. Thus we may obſerve when a woman begins to breed at an early period, as at fifteen, and has her children faſt, that ſhe ſeldom breeds longer than the age of thirty or thirty-five; therefore we may ſuppoſe, either that the parts are then worn out, or that the breeding conſtitution is over. If a woman begins later, as at twenty or twenty-five, ſhe may continue to breed to the age of forty or more; and there are, now and then, inſtances of women, who, not having

conceived before, have had children as late in life as at fifty years or upwards. After that period few women breed, even though they ſhould not have bred before; therefore, there muſt be a natural period to the power of conception. A ſimilar ſtop to propagation may likewiſe take place in other claſſes of animals, probably in the female of every claſs, the period varying according to circumſtances; but ſtill we are not enabled to determine, how far it depends on any particular property of the conſtitution, or of the ovarium alone.

As the female of moſt claſſes of animals has two ovaria, I imagined, that by removing one it might be poſſible to determine how far their actions were reciprocally influenced by each other; from the changes which by compariſon might be obſerved to take place, either by the breeding period being ſhortened, or perhaps, in thoſe animals whoſe nature it is to bring forth more than one at a time, by the number produced at each birth being diminiſhed.

There are two views in which this ſubject may be conſidered. The firſt, that the ovaria, when properly employed, may be bodies determined and unalterable reſpecting the number of young to be produced. In this caſe we can readily imagine, that, when one ovarium is removed, the other may be capable of producing its determined number in two different ways: one, when the remaining ovarium not influenced by the loſs of the other, will produce its allotted number, and in the ſame time: the other, when affected by the loſs, yet the conſtitution demanding the ſame number of young each time of breeding, as if there were ſtill two ovaria; it muſt furniſh double the number it would have been required to ſupply, had both been allowed to remain; but muſt conſequently ceaſe from the performance of its function in half the time. The ſecond view of the ſubject is by ſuppoſing, that there is not originally any fixed number which the ovarium muſt produce, but that the number is increaſed or diminiſhed according to circumſtances; that it is rather the conſtitution at large that determines the number; and that, if one ovarium is removed, the other will be called upon by the conſtitution to perform the operations of both, by which means the animal ſhould produce, with one ova-

rium, the ſame number of young as would have been produced if both had remained.

With an intention to aſcertain thoſe points, as far as I could, I was led to make the following experiment; and for that purpoſe gave pigs a preference to any other animal, as being eaſily managed, producing ſeveral at a litter, and breeding perfectly well under the confinement neceſſary for experiments. I ſelected two females of the ſame colour and ſize, and likewiſe a boar-pig, all of the ſame farrow; and having removed an ovarium from one of the females, I cut a ſlit in one ear, to diſtinguiſh it from the other: they were well fed and kept warm, that there might be no impediment to their breeding; and whenever they farrowed, their pigs were taken away exactly at the ſame age.

About the beginning of the year 1779, they both took the boar; the one which had been ſpayed, earlier than the perfect female. The diſtance of time, however, was not great, and they continued breeding at nearly the ſame times. The ſpayed animal continued to breed till September 1783, when ſhe was ſix years old, which was a ſpace of more than four years. In that time ſhe had eight farrows; but did not take the boar afterwards, and had in all ſeventy-ſix pigs. The perfect one continued breeding till December 1785, when ſhe was about eight years old, a period of almoſt ſix years, in which time ſhe had thirteen farrows, and had in all one hundred and ſixty-two pigs; after this time ſhe did not breed: I kept her till November 1786.

I have here annexed a table of the different times of each farrow, with the number of pigs produced.

SPAYED SOW.

Farrows.	Number of young.	Time.
1	6	Dec. 1779.
2	8	July 1780.
3	6	Jan. 1781.
4	10	Aug. 1781.
5	10	Mar. 1782.
6	9	Sept. 1782.
7	14	May 1783.
8	13	Sept. 1783.
	76	

November following ſhe was put to the boar, but brought no pigs. April 1784, ſhe was again put to the boar, without effect, and never was obſerved to take the boar afterwards, although often with him. November 1784, ſhe was killed.

PERFECT SOW.

Farrows.	Number of young.	Time.
1	9	
2	6	
3	8	
4	13	Dec. 1781.
5	10	June 1782.
6	16	Dec. 1782.
7	13	June 1783.
8	12	Oct. 1783.
	87	

Eleven pigs more than were produced by the ſpayed ſow in her eight farrows.

Farrows.	Number of young.	Time.
9	12	Feb. 1784.
10	16	June 1784.
11	12	Dec. 1784.
12	16	May 1785.
13	19	Dec. 1785.
	75	

After which ſhe bred no more.

The firſt eight farrows were	87
The laſt five farrows were	75
Total	162
The number from the ſpayed one	76
More than farrowed by the imperfect animal . . .	86

It is obſervable, that both ſows rather increaſed in their number each time as they grew older, although not uniformly; the difference between the firſt and laſt in both animals being conſiderable.

From the above table we find, that the ſow with only one ovarium bred till ſhe was ſix years old, from the latter end of 1779 till September 1783, about four years, and in that time brought forth ſeventy-ſix pigs. The perfect animal bred till ſhe was eight years of age; and if conception depended on the ovaria, we might have expected, that ſhe would bring forth double the number at each birth; or, if not, that ſhe would continue breeding for double the time. We indeed find her producing ten more than double the number of the imperfect animal, although ſhe had not double the number of farrows; but this may, perhaps be explained by obſerving that the number of young increaſed as the female grew older, and the perfect ſow continued to breed much longer than the other.

From a circumſtance mentioned in the courſe of this experiment it appears, that the deſire for the male continues after the power of breeding is exhauſted in the female; and therefore does not altogether depend on the powers of the ovaria to propagate; although it may probably be influenced by the exiſtence of ſuch parts.

If theſe obſervations ſhould be conſidered as depending on a ſingle experiment, from which alone it is not juſtifiable to draw concluſions, I have only to add, that the difference in the number of pigs produced by each was greater than can be juſtly imputed to accident, and is a circumſtance certainly in favour of the univerſality of the principle I wiſhed to aſcertain*.

From this experiment it ſeems moſt probable, that the ovaria are from the beginning deſtined to produce a fixed number, beyond which they cannot go, although circumſtances may tend to diminiſh that number; but that the conſtitution at large has no power of giving to one ovarium the power of propagating equal to both; for in the preſent experiment, the animal with one ovarium produced ten pigs leſs than half the number brought forth by the ſow with both ovaria. But that the conſtitution has ſo far a power of influencing one ovarium, as to make it produce its number in a leſs time than would probably have been the caſe if both ovaria had been preſerved, is to be inferred from the above recited experiment.

* It may be thought by ſome, that I ſhould have repeated this experiment; but an annual expence of twenty pounds for ten years, and the neceſſary attention to make the experiment complete, will be a ſufficient reaſon for my not having done it.

ON THE STRUCTURE OF THE PLACENTA.

THE connection between the mother and fœtus in the human ſubject, has in every age, in which ſcience has been cultivated, called forth the attention of the anatomiſt, the phyſiologiſt, and even the philoſopher; but both that connection and the ſtructure of the parts which form the connection, were unknown till about the year 1754. The ſubject is certainly moſt intereſting, and the diſcovery important; and it is my intention, in the following pages, to give ſuch an account of it as I hope may be acceptable to the public *; while, at the ſame time, I eſtabliſh my own claim to the diſcovery. But that I may not ſeem to arrogate to myſelf more merit than I am entitled to, let me, in juſtice to another perſon, relate what follows.

The late indefatigable Dr. Mc. Kenzie, about the month of May 1754, when aſſiſtant to Dr. Smellie, having procured the body of a pregnant woman, who died undelivered at the full term, had injected both the veins and arteries with particular ſucceſs; the veins being filled with yellow, the arteries with red†.

Having opened the abdomen, and expoſed the uterus, he made an inciſion into the fore part, quite through its ſubſtance, and came to what ſeemed to be an irregular maſs of injected matter. The appearance being new, he proceeded no further, and greatly obliged me, by deſiring my attendance to examine parts, in which the appearances were ſo uncommon. The examination was made in his preſence, and in the preſence of ſeveral

* This Paper was read at the Royal Society; but as the facts had, before that time, been given to the public, it was not publiſhed in the Philoſophical Tranſactions.

† Dr. Mc. Kenzie being then an aſſiſtant to the late Dr. Smellie, the procuring and diſſecting this woman, without Dr. Smellie's knowledge, was the cauſe of a ſeparation between them: for the leading ſteps to ſuch a diſcovery could not be kept a ſecret. The winter following, Dr. Mc. Kenzie began to teach midwifery in the Borough of Southwark.

other gentlemen, whoſe names I have now forgotten; but I have reaſon to believe that ſome are ſettled in this country, who I hope will have an opportunity of peruſing this publication*.

I firſt raiſed, with great care, a part of the uterus from the irregular maſs, and in doing this, obſerved regular pieces of wax paſſing obliquely between it and the uterus, which broke off, leaving part attached to that maſs; and on attentively examining the portions towards the uterus, they plainly appeared to be a continuation of the veins paſſing from it to this ſubſtance, which proved to be placenta.

I likewiſe obſerved other veſſels, about the ſize of a crow-quill, paſſing in the ſame manner, although not ſo obliquely; theſe alſo broke upon ſeparating the placenta and uterus, leaving a ſmall portion on the ſurface of the placenta; and on examination they were diſcovered to be continuations of the arteries of the uterus. My next ſtep was to trace theſe veſſels into the ſubſtance of what appeared placenta, which was firſt attempted in a vein; but that ſoon loſt the regularity of a veſſel, by terminating at once upon the ſurface of the placenta in a very fine ſpongy ſubſtance; the interſtices of which were filled with the yellow injected matter. This termination being new, I repeated the ſame kind of examination on other veins, which always led me to the ſame terminations, never entering the ſubſtance of the placenta in the form of a veſſel. I then examined the

* If I ſhould be ſo fortunate as to have this publication fall into any of thoſe gentlemens hands, I hope they will favour me with their opinion of my ſtate of the facts, which led to the diſcovery.

It may be ſuſpected by ſome, (but none I hope to whom I have the pleaſure of being known) that I am not doing Dr. Mc. Kenzie juſtice, and am perhaps ſuppreſſing ſome part of that ſhare of the diſcovery to which he is entitled. This idea, (if ever it ſhould ariſe) I may, probably not be able to remove; but I hope it will alſo be ſeen, that I myſelf have given riſe to it; believing, if I had been ſo inclined, that I might have ſuppreſſed Dr. Mc. Kenzie's name altogether, without ever running the hazard of being detected. I was indeed ſo tenacious of my claim to the diſcovery, that I wrote this account in Dr. Mc. Kenzie's life-time, with a deſign to publiſh it; and often communicated my intentions to Dr. George Fordyce, who I knew was very intimate with the Doctor, in conſequence of both teaching in the ſame place, and making many experiments together; therefore he is a kind of collateral witneſs, that what I now publiſh is the ſame account which I gave in Dr. Mc. Kenzie's life-time.

arteries, tracing them in the ſame manner towards the placenta, and found that, having made a twiſt, or cloſe ſpiral turn upon themſelves, they were loſt on its ſurface. On a more attentive view, I perceived that they terminated in the ſame way as the veins; for oppoſite to the mouth of the artery, the ſpongy ſubſtance of the placenta was readily diſtinguiſhed with the red injection intermixed.

Upon cutting into the placenta I diſcovered, in many places of its ſubſtance, yellow injection, in others red, and in many others theſe two colours mixed. The ſubſtance of the placenta, now filled with injection, had nothing of the vaſcular appearance, nor that of extravaſation, but had a regularity in its form which ſhowed it to be naturally of a cellular ſtructure, fitted to be a reſervoir for blood.

I perceived likewiſe, that the red injection of the arteries, (which had been firſt injected) had paſſed out of the ſubſtance of the placenta into ſome of the veins leading from the placenta to the uterus, mixing itſelf with the yellow injection; and that the ſpongy chorion, called the decidua, by Dr. Hunter, was very vaſcular, its veſſels going to and from the uterus, being filled with the different coloured injections.

After having conſidered theſe appearances, it was not difficult for me to determine the real ſtructure of the placenta and courſe of the blood in theſe parts: but the company, prejudiced in favour of former theories, combated my opinion; and it was even diſputed, whether or not theſe curling arteries could carry red blood. After having diſſected the uterus, with the placenta and membranes, and made the whole into preparations, tending to ſhow the above facts, I returned home in the evening, and communicated what I had diſcovered to my brother, Dr. Hunter, who at firſt treated it and me with good humoured raillery; but on going with me to Dr. Mc. Kenzie's he was ſoon convinced of the fact. Some of the parts were given to him, which he afterwards ſhowed at his lectures, and probably they ſtill remain in his collection.

Soon after this time, Dr. Hunter and I procured ſeveral placentæ, to diſcover if, after delivery, the termination of the veins, and the curling arteries, could be obſerved: they were diſcernible almoſt in every one; and

by puſhing a pipe into the placenta, we could fill not only its whole ſubſtance, but alſo the veſſels on that ſurface which was attached to the uterus, with injection.

The facts being now aſcertained, and univerſally acknowledged, I conſider myſelf as having a juſt claim to the diſcovery of the ſtructure of the placenta, and its communication with the uterus; together with the uſe ariſing from ſuch ſtructure and communication, and of having firſt demonſtrated the vaſcularity of the ſpongy chorion.

It is not neceſſary at preſent to enter into the various opinions which have been formed on this ſubject; becauſe, whatever they were, they could not be juſt, the ſtructure of the parts not being known: neither ſhall I endeavour to give a complete deſcription of all the parts immediately connected with uterine geſtation, but content myſelf with deſcribing the ſtructure of the placenta, as far as it has any relation to the uterus and child; and with explaining the connection between the two; leaving the reader to examine what has been ſaid upon this ſubject by others, eſpecially by Dr. Hunter, in that very accurate and elaborate work which he has publiſhed on the Gravid Uterus, in which he has minutely deſcribed, and accurately delineated the parts, without mentioning the mode of diſcovery.

The neceſſary connection ſubſiſting in all animals between the mother and fœtus, for the nouriſhment of the latter, as far as I know, takes place in two ways. In ſome it is continued, and ſubſiſts through the whole term of geſtation; in others the union is ſoon diſſolved; but an apparatus is provided, which at once furniſhes what is ſufficient for the ſupport of the animal till it comes forth.

The firſt of theſe are the viviparous, the ſecond the oviparous animals, both of which admit of great variety in the mode by which the ſame effect is produced*. In the firſt diviſion is included the human ſpecies,

* It may be remarked here, that the oviparous admit of being diſtinguiſhed into two claſſes, one where the egg is hatched in the belly, as in the viper, which has been commonly called viviparous; the others, where the eggs have been firſt laid and then hatched, which is the claſs commonly called oviparous, ſuch as all the bird tribe; and many others, as ſnakes, lizards, &c.

which alone will engage our preſent attention. But before I deſcribe this connection, it may be neceſſary that the reader ſhould underſtand my idea of generation: I ſhall therefore refer him to what I have ſaid upon that ſubject in my account of the free martin*.

In the human ſpecies, the anatomical ſtructure of the mother and embryo, relative to fœtation, being well known, it will only be neceſſary fully to deſcribe the nature of the connection between them, which is formed by the intermediate ſubſtance, called placenta. For this purpoſe we muſt firſt conſider the placenta as a common part; next, the uterus as belonging to the mother, yet having an immediate connection with the placenta, from which the nouriſhment of the fœtus is to be derived; which will lead us laſtly to a conſideration of thoſe peculiarities of ſtructure, by means of which the fœtus is to receive its nouriſhment, and which likewiſe conſtitutes its immediate communication with the placenta. It is the ſtructure of this intermediate ſubſtance, and its connection with the child and the uterus of the mother, which have hitherto been ſo little underſtood; and without an accurate knowledge of which, it was impoſſible any juſt idea could be formed of its functions.

The placenta is a maſs lying nearly in contact with the uterus; indeed it may in ſome degree be ſaid to be in continuity with a part of its internal ſurface. On the ſide applied to the uterus the placenta is lobulated, having deep irregular fiſſures. It is probable, from this ſtructure of the placenta, that the uterus has an inteſtine motion while in the time of uterine geſtation; not an expulſive one, which thoſe lobes of the placenta allows of; but all theſe lobes are united into one uniform ſurface on that ſurface next to the child, where its umbilical veſſels ramify. When we cut into the placenta, its whole ſubſtance appears to be little elſe than a network, or ſpongy maſs, through which the blood-veſſels of the fœtus ramify, and indeed ſeems to be principally formed by the ramifications of thoſe veſſels; it exhibits hardly any appearance of connecting membrane;

* Vide page 55.

but we cannot readily ſuppoſe it to be without ſuch a membrane, as there is ſo much regularity in its texture. The cells, or interſtices of each lobe communicate with one another, even much more freely than thoſe of the cellular membrane in any other part of the body; ſo that whatever fluid will paſs in at one part, readily diffuſes itſelf through the whole maſs of lobe; and all the cells of each lobe have a communication at the common baſe.

This ſtructure of the placenta, and its reciprocal communication with the two bodies with which it is immediately connected, form the union between the mother and fœtus for the ſupport of the latter. Prior to the time I have mentioned above, anatomiſts ſeem to have been wholly unacquainted with the true ſtructure of placenta. By notes taken from Dr. Hunter's lectures, in the winter 1755-6, it appears that he expreſſed himſelf in the following manner*. "The ſubſtance of the placenta is a fleſhy maſs, which ſeems to be formed entirely of the veſſels of the umbilical rope." In another part, mentioning the appearances when injected, he ſays; "and upon a ſlight putrefaction coming on, you will find the whole appearing like a maſs of veſſels": then ſays, "there is always a white uninjected ſubſtance between the veſſels; but whether lymphatics or what I cannot tell." This uninjected ſubſtance, mentioned by Dr. Hunter, is what forms the cellular ſtructure.

The placenta ſeems to be principally compoſed of the ramifications of the veſſels of the embryo, and may have been originally formed in conſequence of thoſe next to the uterus laying hold by a ſpecies of animal attraction of the coagulable lymph which lines the uterus. It might take place in a manner reſembling what happens when the root of a plant ſpreads on the ſurface of moiſt bodies; with this difference, that in the

* Theſe quotations were taken from Mr. Galhie's MS. of Dr. Hunter's lectures, who is one of the gentlemen that favoured Dr. Hunter, upon a former occaſion, with the uſe of his notes. Vide Dr. Hunter's Commentaries.

present inſtance the veſſels form the ſubſtance through which they ramify, as in the caſe of granulations.

At the time, or perhaps before the female ſeed enters the uterus, coagulable lymph, from the blood of the mother, is thrown out every where on its inner ſurface, either from the ſtimulus of impregnation taking place in the ovarium, or in conſequence of the ſeed being expelled from it. But I think the firſt the moſt probable ſuppoſition; for we find in extra-uterine caſes, that the decidua is formed in the uterus, although the ovum never enters it; which is a proof that it is produced by the ſtimulus of impregnation in the ovarium; and that it is prior to the enterance of the ovum into the uterus. When it has entered the uterus, it attaches itſelf to that coagulable lymph, by which, being covered and immediately ſurrounded*, there is formed a ſoft pulpy membrane, the decidua, which, I believe, is peculiar to the human ſpecies, and to monkeys, I never having found it in any other animal. That part which covers the ſeed or fœtus, where it is not immediately attached to the uterus, and likewiſe forms a membrane, was diſcovered by Dr. Hunter, and is by him called decidua reflexa†. The whole of this coagulable lymph continues to be a living part for the time; the veſſels of the uterus ramify upon it; and where the veſſels of the fœtus form the placenta, there the veſſels of the uterus, after paſſing through the decidua, open

* This is ſomewhat ſimilar to another operation in the animal œconomy. If an extraneous living part is introduced into any cavity, it will be immediately encloſed with coagulable lymph. Thus we find worms incloſed, and hydatids, that have been detached, afterwards incloſed; but in thoſe caſes this is a conſequence of the preſſure of the extraneous body; whereas in the uterus it is preparatory.

† The placenta is certainly a fœtal part, and is formed on the inſide of the ſpongy chorion, or decidua. How far the decidua reflexa is a uterine part, I do not yet know; if it is, then the ovum muſt be placed in a doubling of the coagulum, which forms the decidua; but if the ovum is attached to the inſide of the decidua, then the decidua reflexa is belonging to the fœtus.

into the cellular ſubſtance of the placenta, as before deſcribed. As this membrane lines the uterus and covers the ſeed, it is ſtretched out, and becomes thinner and thinner, as the uterus is diſtended by the fœtus growing larger, eſpecially that part of it, called decidua reflexa, which covers the fœtus; as there it cannot poſſibly acquire any new matter, except we could ſuppoſe that the fœtus aſſiſted in the formation of it. This membrane is moſt diſtinct where it covers the chorion; for where it covers the placenta it is blended with coagula in the great veins that paſs obliquely through it, more eſpecially all round the edge, where innumerable large veins come out; but the chorion and decidua can be eaſily diſtinguiſhed from one another, the decidua being leſs elaſtic.

From the deſcription now given, I think we are juſtified in ſuppoſing the placenta to be formed entirely by the fœtus, which is farther confirmed by extra-uterine caſes, and by the formation of the membrane in the egg; there being no living organic part to furniſh them; and the decidua we muſt ſuppoſe to be a production of the mother; of both which, the circumſtance of the decidua paſſing between the placenta and uterus, may be conſidered as an additional proof. For if the veſſels of the fœtus branched into a part of the decidua, we might conceive the whole placenta to be formed from that exudation; the portion of it, where the veſſels had ramified, like the roots of a plant, becoming thicker than the reſt, and forming the placenta. If that were the caſe, this membrana decidua, when traced from the parts diſtinct, and at a diſtance from the placenta, ſhould be plainly ſeen paſſing into its ſubſtance all round at the edges, as a continuation of it. But the fact is quite otherways; for the decidua can be diſtinctly traced between the placenta and uterus, hardly ever paſſing between the lobuli: the veſſels of the fœtus never entering into it, and of courſe none of them ever coming in abſolute contact with the uterus. But what may be conſidered as ſtill a ſtronger proof that the decidua is furniſhed by the uterus, is, that in caſes of extra-uterine conception, where the fœtus is wholly in the ovarium or Fallopian tube, we find the uterus lined with the decidua,

having taken on the uterine action; but no placenta, that being formed by the fœtus, and therefore in the part which contained it.

The veſſels of the fœtus adhering, by the intervention of the decidua, to a certain portion of the uterus when both are yet ſmall, as the uterus increaſes in every part of its ſurface during the time of uterine geſtation, we muſt ſuppoſe that this ſurface of adheſion increaſes alſo; and that by the elongation of thoſe veſſels of the fœtus in every direction, this ſubſtance ſhould likewiſe be increaſed in every direction: this is in ſome degree the caſe, yet the placenta does not occupy ſo much of the enlarged ſurface of the uterus as one at firſt would expect.

The veſſels of the uterus in the time of the geſtation, are increaſed in ſize nearly in a proportion equal to the increaſed circumference of the uterus, and conſequently in a proportion much greater than the real increaſe of its ſubſtance. But when we reflect that the uterus ought not to be conſidered as hollow, but as a body nearly ſolid, on account of its contents, which derive ſupport from this ſource, and that a much greater quantity of blood muſt neceſſarily paſs than what is required for the ſupport of the viſcus itſelf, we cannot be at a loſs to account for the greatly increaſed ſize of its veſſels.

The arteries which are not immediately employed in conveying nouriſhment to the uterus, go on towards the placenta, and proceeding obliquely between it and the uterus, paſs through the decidua without ramifying; juſt before they enter the placenta, after making two or three cloſe ſpiral turns upon themſelves, they open at once into its ſpongy ſubſtance without any diminution of ſize, and without paſſing beyond the ſurface, as above deſcribed. The intention of theſe ſpiral turns would appear to be that of diminiſhing the force of the circulation in the veſſels as they approach the ſpongy ſubſtance of the placenta, and is a mechaniſm calculated to leſſen the quick motion of the blood in a part where a quick motion was not required. Theſe curling arteries at this termination are in general about half the ſize of a crow's quill, and ſometimes larger.

The veins of the uterus appropriated to bring back the blood from the placenta, commence from this ſpongy ſubſtance by ſuch wide beginnings, as are more than equal to the ſize of the veins themſelves. Theſe veins paſs obliquely through the decidua to the uterus, enter its ſubſtance obliquely, and immediately communicate with the proper veins of the uterus. The area of theſe veins bear no proportion to their circumference, the veins being very much flattened.

This ſtructure of parts points out at once the nature of the blood's motion in the placenta; but as this is a fact but lately aſcertained, a juſt idea may perhaps be conveyed by ſaying, that it is ſimilar, as far as we yet know, to the blood's motion through the cavernous ſubſtance of the penis.

The blood, detached from the common circulation of the mother, moves through the placenta of the fœtus; and is then returned back into the courſe of the circulation of the mother to paſs on to the heart.

This ſtructure of the placenta, and its communication with the uterus, leads us a ſtep further in our knowledge of the connection between the mother and fœtus; the blood of the mother muſt paſs freely into the ſubſtance of the placenta, and the placenta moſt probably will be conſtantly filled; the turgidity of which will aſſiſt to ſqueeze the blood into the mouths of the veins of the uterus, that it may again paſs into the common circulation of the mother: and as the interſtices of the placenta are of much greater extent than the arteries which convey the blood, the motion of the blood in that part muſt be ſo much diminiſhed as almoſt to approach to ſtagnation; ſo far and no further does the mother appear to be concerned in this connection.

The fœtus has a communication with the placenta of another kind. The arteries from the fœtus paſs out to a conſiderable length, under the name of the umbilical arteries, and when they arrive at the placenta, ramify upon its ſurface, ſending into its ſubſtance branches which paſs through it, and divide into ſmaller and ſmaller, till at laſt they terminate in veins;

theſe uniting, become larger and larger, and end in one, which at laſt communicates with the proper circulation of the fœtus.

This courſe of veſſels, and the blood's motion in them, is ſimilar to the courſe of the veſſels and the motion of the blood in other parts of the body.

In addition to what I have ſaid about the connection between the mother and child, in natural caſes, it is neceſſary to obſerve, that though the uterus is appropriated for the ſupport of the fœtus, as beſt fitted for that purpoſe, yet it is not eſſential to its growth; as any other part in which the child may be ſituated, is capable of receiving the ſame proviſionary ſtimulus for ſupplying it with nouriſhment as the uterus; and this, I believe, is peculiar to generation. This prompts me to make the following obſervations upon the different ſituations of the fœtus in extra-uterine caſes, which are extraordinary, happen ſeldom, and when they do occur, are often attended with ſo many hinderances to critical inveſtigation, as hardly to allow of thorough or ſatisfactory information.

Such caſes are readily diſtinguiſhed from natural ones, by the uterus being found entire and empty; and they may be divided into three different kinds, according to the ſituation of the fœtus in the ovarium, Falopian tube, or in the cavity of the abdomen.

From a want of the appearances which uſually attend the natural proceſs, the inveſtigation of extra-uterine caſes is attended with conſiderable difficulty. For where uncommon actions have taken place, as well as in caſes of diſeaſe, the natural texture of the parts is very much altered, and appears to be loſt; not only by the parts themſelves being enlarged, but from having a great deal of new matter ſuperadded to them, by which they loſe their natural diſtinctneſs, and become leſs fitted for examination than thoſe which only have a relation to them, and which preſerve their natural actions peculiar to that ſtate.

From theſe difficulties, and a want of accuracy in thoſe who made the examination, it is not at preſent clear, with reſpect to many of the extra-uterine caſes upon record, whether they were ovarian caſes, Falopian tube caſes, or abdominal caſes; when, if they had been acquainted with the

principle in which they differ, nothing could have been more eaſy than to diſtinguiſh them. It is not difficult, perhaps, at the very firſt view, to diſtinguiſh an abdominal caſe from either of the two firſt: for if the ovaria and Falopian tube are entire, natural, and can be well diſtinguiſhed to be as thoſe parts are when the circumſtances are natural, then we may be ſure it is an abdominal caſe. Appearances, however, may not in all caſes be diſtinct; but the parts may adhere, or be otherwiſe rendered ſo obſcure, that an abdominal caſe might be confounded with either of the two firſt; therefore it is eſſential to have a characteriſtic difference eſtabliſhed between the two firſt, and the third.

The invariable difference between the two firſt, and the abdominal caſes, will be in the veſſels by which the child is nouriſhed; for the arteries and veins belonging to the part in which the child is contained muſt be enlarged; which, being the increaſe of a natural part, will be readily aſcertained, and the nature of the caſe as readily determined. We may lay it down as a principle, that when the ſpermatic artery, and veins of either ſide, is enlarged in an extra-uterine caſe, that the fœtus is in the ovarium or Falopian tube; ſince there are no other blood-veſſels which ſupply theſe parts; and if any other ſyſtem of veſſels, as the meſenteric, are increaſed in ſize, while the ſpermatic are in a natural ſtate, we may, with equal certainty, conclude the fœtus to be contained in the general cavity of the belly. As this becomes the great criterion, and as the ſituation and time will not always allow very nice inveſtigation on the ſpot, where the perſon employed has an opportunity of taking away the parts concerned, I would adviſe his taking along with them the aorta and vena cava, cut through above the origins of the ſpermatic veſſels.

Plate XIII

Wm. Bell del.

Wm. Skelton sculp.

PLATE XIII.

A part of the uterus at the ninth month of utero-geſtation, with a portion of the placenta, to ſhow the mode in which the blood-veſſels of the mother communicate with it.

A The ſubſtance of the uterus, ſeparated from the placenta, and turned back.

B The ſurface of the placenta by which it is attached to the uterus, covered by the decidua.

C The angle of reflection, at which the uterus is turned back upon itſelf.

D The edge of the placenta.

E The decidua covering the chorion.

Upon the ſurface of the uterus are to be ſeen the veins or ſinuſes, running in an oblique direction, filled with wax, and broken off where they paſs through the decidua.

a a a a The arteries injected and broken off as they paſs from the uterus to the placenta.

b b b b The continuation of theſe arteries, which make ſeveral ſpiral turns as they dip into the decidua, and afterwards terminate on the ſurface of the placenta.

c c c c The veins injected and broken off where they paſs into the ſubſtance of the uterus.

d d d d The correſponding portions of the ſame veins, where they paſs from the placenta through the decidua.

e e e e The blood-veſſels, ramifying upon the decidua, broken off from the uterus.

OBSERVATIONS ON THE PLACENTA OF THE MONKEY.

Monkeys always copulate backwards; this is performed ſometimes when the female is ſtanding on all-fours; and at other times the male brings her between his thighs when he is ſitting, holding her with his fore paws.

The female has her regular periods for the male, but ſhe has commonly too much complaiſance ever to refuſe him. They carry this ſtill further, for they receive the male when with young, even when pretty far gone; at leaſt this was the caſe with one of which I am going to give an account.

A female monkey, belonging to Mr. Enderſbay, in the ſummer 1782, had frequently taken the male. The keeper obſerved, that after the 21ſt of June ſhe became leſs lively than uſual, although it was not ſuſpected that ſhe had conceived; but ſome time after appearing to be bigger in the belly, it created a ſuſpicion of her being with young. Great attention was paid to her, and great care was taken of her. She went on gradually increaſing in ſize; and at laſt ſomething was obſerved to move in her belly at particular times, and the motion could even be felt through the abdominal muſcles. She became indolent, and did not like to leap or perform her uſual feats of activity. Towards the latter part of the time, they perceived the breaſt and nipple to have become rather fuller; and that a kind of water could be ſqueezed out at the nipple. Some time before ſhe brought forth, ſhe became red about the hips and poſteriors; which redneſs extended to the inſide of the thighs, it being now certain that ſhe was with young. I deſired that ſhe might be particularly attended to when there were ſigns of approaching delivery, both on her own account and that of the young one, and requeſted the afterbirth might be carefully preſerved, as that part would aſſiſt to aſcertain the mode of uterine geſtation. Theſe directions were attentively followed; and when

in labour it was obſerved, that ſhe had regular pains; that when the young one was in part come into the world, ſhe aſſiſted herſelf with her fore paws; and that it came with the hind parts firſt. This happened on the 15th of December 1782, in all about ſix months after conception; and when ſhe brought forth her young one, it ſhowed ſigns of life, but died immediately, owing, probably, to the unfavourable mode of its being brought into the world. When delivered, ſhe took the young one up, and although it was dead, claſped it to her breaſt.

The afterbirth was preſerved entire, and was perfectly fit for examination. It conſiſted of placenta, with the membranes and navel-ſtring, which all very much reſembled the correſponding parts in the human ſubject, as will now be deſcribed.

The placenta had the appearance of being divided into two oblong bodies, united by their edges, each terminating in an obtuſe point at the other end, which were of courſe at ſome little diſtance from one another.

It is probable, that theſe two points were placed towards the openings of the Fallopian tubes, where the uterus aſſumes a form reſembling two obtuſe horns.

The two lobes abovementioned, were made up of ſmaller ones, united cloſely at their edges, which were more apparent and diſtinct at ſome parts than at others. Some of theſe lobes were divided by fiſſures which ſeem to be derived from one centre; while there were others near the edges, paſſing in a different direction; in which fiſſures are placed veins or ſinuſes that receive the blood laterally from the lobes. The ſubſtance of the placenta ſeems to be cellular, as in the human ſubject; this ſtructure allows a communication to be kept up between different parts of each lobe, and the ſinuſes allowing of a communication between the different lobes of which the placenta is compoſed, the blood paſſes into the fiſſures before it enters the veins; in which reſpect it differs from the human placenta.

The arteries from the uterus, on the ſurface of the placenta, were viſible, but too ſmall to be injected; I cannot therefore ſay how they terminated in the placenta.

The principal veins arofe in general from the fiffures beginning from the furface, as in the human placenta; but befides thefe, there were other fmall ones; all which, we may fuppofe, pafs through the decidua and enter the fubftance of the uterus, moft probably in the fame way as in the human.

The membranes are the amnios, the chorion, and the membrana decidua. Thefe appear to be much the fame as in the human, except that the decidua is confiderably thicker, efpecially where it paffes between the uterus and the placenta.

The navel-ftring in the monkey is not proportionally fo long as in the human; and is very much, and very regularly twifted.

There is no urachus, and of courfe no allantois, not even the fmall ligament that appears to be a drawing in of the bladder at its attachment to the navel, the bladder here being rounded.

OBSERVATIONS ON THE GILLAROO-TROUT, COMMONLY CALLED, IN IRELAND, THE GIZZARD-TROUT.

ONE of the digeſtive organs of the gillaroo-trout being ſo very remarkable as to have given name to the fiſh, and to have been conſidered as its diſtinguiſhing characteriſtic; it is my intention to inquire whether its reſemblance to a gizzard be ſufficiently ſtrong to render the term of gizzard-trout a proper appellation, and what place its ſtomach ought to hold among the correſponding organs of other animals. For this purpoſe it will be neceſſary to ſtate certain facts connected with the ſubject; and take a general view of the varieties which occur in the digeſtive organs in different animals.

The food of animals may be divided into two kinds, what does, and what does not, require maſtication to facilitate digeſtion. The fleſh of animals is of the latter kind; but grain, and many other ſubſtances which ſerve for aliment, require a previous grinding or trituration; and therefore animals living on this kind of food are furniſhed with organs for that purpoſe. Granivorous quadrupeds have the two powers, for maſtication and digeſtion, ſeparate or diſtinct from one another; the firſt being executed by teeth, which ſerve as ſo many grindſtones for reducing their food to ſmaller parts, before it is conveyed into the ſtomach for digeſtion; but the form of theſe teeth varies very conſiderably in different animals, although the food be the ſame. This grinding alſo fits it for deglutition: for neither grain nor herbs could be ſwallowed without having firſt been maſticated. When ſo prepared, it is, with regard to the digeſtive power, rendered ſimilar to animal food: therefore in many of the granivorous, the ſtomach reſembles that of the carnivorous animals; and whenever the ſtomach in the granivorous quadruped departs from this general rule,

there is a peculiarity in the operations of digeſtion. Birds that live upon ſubſtances, for the digeſtion of which trituration is indiſpenſably neceſſary, have the powers of maſtication and digeſtion united in one part, the gizzard, which is particularly conſtructed for that purpoſe; but is more uniform in its conſtruction than the teeth, varying only by being ſtronger or weaker in its powers; therefore the genus of birds exhibits leſs variety, reſpecting the organs relating to digeſtion, than the quadruped. In granivorous birds, therefore, one ſingle organ anſwers both to the teeth and ſtomach of granivorous quadrupeds, and conſequently the gizzard alone of birds will as clearly point out the food of the ſpecies as both teeth and ſtomach together, in thoſe animals in which the two offices of maſtication and digeſtion are not performed together in the ſame part.

As it appears to be the difference of ſtomachs only, that fits birds for their different kinds of food, as there is little difference in conſtruction, excepting only in ſtrength; and as the food of the different ſpecies is of every kind, from the hardeſt grain to the ſofteſt animal matter, we may conclude, that every gradation of the ſtomach is to be found among them, from the true gizzard, which is one extreme, to the mere membranous ſtomach, which is the other. In conſequence of this, it muſt be as difficult to determine the exact limits of the two different modes of conſtruction, to which the names of gizzard and ſtomach ſpecifically belong, as, in any other caſe, to diſtinguiſh proximate ſteps in the ſlow and imperceptible gradations of Nature.

The two extremes of true gizzard, and membranous ſtomach, are eaſily defined; but they run ſo into each other, that the end of one and the beginning of the other is quite imperceptible. Similar gradations are obſervable in the food; the kinds ſuited to the two extremes mixing together in different proportions, adapted to the intermediate ſtates of ſtomach.

A true gizzard is compoſed of two ſtrong muſcles placed oppoſite, and acting upon each other, like two broad grindſtones. Theſe muſcles are joined together at their ſides by a middle tendon, into which the muſcular fibres are inſerted, and which forms the narrow anterior and poſterior ſides

of the flat quadrangular cavity, in which the grinding is performed. The upper end of this cavity is occupied by the termination of the œſophagus, and the beginning of the inteſtine. The lower end conſiſts of a thin muſcular bag connecting the edges of the two muſcles together.

By theſe two more ſoft and flexible ſubſtances being thus interpoſed between the two ſtrong grinding muſcles, a double advantage is gained; for whilſt one gives an eaſy paſſage to the œſophagus and gut, when both act together they ſerve in ſome degree as a hinge, on which the two muſcles may be ſaid to move, by the middle tendon allowing of a free motion of the grinding ſurfaces on each other, which is neceſſary for the comminution of food.

The two flat lateral ſides of the grinding cavity are lined with a thick horny ſubſtance ſimilar to a hard and thick cuticle: the narrow anterior and poſterior tendinous parts are alſo lined with a cuticle, but not ſo ſtrong as the former: this horny ſubſtance is gradually loſt at one end in a very thin cuticle, which lines the paſſages of the œſophagus and inteſtine for a little way; and at the other end is loſt in the ſame manner in the membranous bag.

The two large muſcles may be conſidered as a pair of jaws, whoſe teeth are occaſionally ſupplied, being ſmall rough ſtones or pebbles which the animal ſwallows; who, from the feeling of the tongue, can diſtinguiſh ſuch as are proper, from thoſe which are not; inſtantly dropping out of its mouth ſuch as are ſmooth and otherwiſe unfit for the purpoſe.

Some birds, with gizzards, have alſo a craw or crop, which ſerves as a reſervoir, and for ſoftening the grain; but as all of them have not this organ, it is not to our preſent purpoſe.

There are other animals, beſides this claſs of birds, which maſticate their food in the ſtomach, but teeth are placed there by Nature: of this kind are crabs and lobſters.

The gradation from gizzard to ſtomach is made by the muſcular ſides becoming weaker and weaker, and the food keeps pace with this change, varying gradually from vegetable to animal. In one point of view, therefore, food may be conſidered as a firſt principle, with reſpect to which the

digeſtive organs, with their appendages, act but as ſecondary parts; being adapted to and determined by the food, as the primary object.

We find then that in all granivorous animals, there is an apparatus for the maſtication of the food, although often differing in conſtruction and ſituation. But in true carnivorous animals, of whatever tribe, maſtication not being ſo neceſſary, they have no apparatus for that purpoſe. The teeth of ſuch quadrupeds, as are carnivorous, ſerve chiefly to procure food and prepare it for deglutition. The ſame thing is performed in the true carnivorous birds, by their beak and talons, whoſe office it is to procure the aliment and fit it for deglutition, correſponding in this reſpect with the teeth of the others. Applying this opinion to fiſh, it ſeems, at firſt ſight, as if there were no occaſion in them for that variety of ſtructure in the digeſtive organs, as is found in the beforementioned quadrupeds and birds; the food of fiſh being principally of one ſort, namely, animal; which, however, with regard to the digeſtive powers, is to be diſtinguiſhed into two kinds, viz. common ſoft fiſh and ſhell-fiſh. Such fiſh as live on the firſt kind, have, like the carnivorous quadrupeds and birds, no apparatus for maſtication; their teeth being intended merely for catching the food and fitting it to be ſwallowed. But the ſhells of the ſecond kind of food render ſome degree of maſticating power neceſſary, to fit it for its paſſage either into the ſtomach or through the inteſtines; and accordingly we find in certain fiſh a ſtructure ſuited to the purpoſe.

Thus the mouth of the wolf-fiſh is almoſt paved with teeth, by means of which it can break ſhells to pieces, and fit them for the œſophagus of the fiſh; and ſo effectually diſengage the food from them, that though it lives upon ſuch hard food, the ſtomach does not differ from that of other fiſh: the organs of maſtication and digeſtion, therefore, in this animal, exactly correſpond to thoſe of many granivorous quadrupeds.

Other fiſh, on the contrary, approach nearer to the ſtructure of birds, by having their ſtomach furniſhed to a certain degree, with a maſticating power, which in many is very imperfect, compared with the gizzards of fowls. Perhaps the difference is only what the difference of food will

properly allow; as in fiſh which have this power, the food being ſtill animal, and in general but imperfectly covered with the ſhell, it probably requires only to be broken; perhaps hardly that, for the mere purpoſes of digeſtion, as food is digeſted when introduced into the ſtomach in ſilver balls with only a few ſmall holes; but it may be neceſſary to fit the ſhells for paſſing along the inteſtines after the fiſh is digeſted. In the bulla lignaria of Linnæus, this apparatus is more perfect, conſiſting of two bones, which we muſt ſuppoſe capable of grinding hard ſhells; but the food of granivorous birds requires to be ground into a kind of meal.

Of all the fiſh I have ſeen, the mullet is the moſt complete inſtance of this ſtructure; its ſtrong muſcular ſtomach being evidently adapted, like the gizzard of birds, to the two offices of maſtication and digeſtion. The ſtomach of the fiſh now before us holds the ſecond place.

But ſtill neither of theſe ſtomachs can be juſtly ranked as gizzards, ſince they want ſome of the moſt eſſential characters, viz. a power and motion fitted for grinding, and the horny cuticle. The ſtomach of the Gillaroo-trout is, however, more globular than that of moſt fiſh, better adapted for ſmall food, and endued with ſufficient ſtrength to break the ſhells of ſmall ſhell-fiſh; which will probably be beſt done by having more than one in the ſtomach at a time, and alſo by taking pretty large and ſmooth ſtones into the ſtomach, which will anſwer the purpoſe of breaking; but not ſo well that of grinding; nor will they hurt the ſtomach as they are ſmooth, when ſwallowed; but this ſtomach can ſcarcely poſſeſs any power of grinding, as the whole cavity is lined with a fine villous coat, the internal ſurface of which appears every where to be digeſtive, and by no means fitted for maſtication.

The ſtomach of the common ſtream-trout is exactly of the ſame ſtructure with that of the gillaroo; but its coat not ſo thick by two-thirds.* How far this difference in thickneſs of ſtomach is ſufficient to form a

* The common ſtream-trout ſwallows ſhell-fiſh, and alſo pretty large ſmooth ſtones, which ſerve as kind of ſhell-breakers.

diſtinct ſpecies, or barely a variety of the ſame, is only to be determined by experiment.*

The œſophagus in the trout is conſiderably longer and ſmaller than in many other claſſes of fiſh.

The inteſtines are ſimilar to thoſe of the ſalmon, herring, ſprat, &c.

The pancreas is appendiculated.†

The teeth ſhow them to be fiſh of prey.

So far as we are led to determine by analogy, we muſt not conſider the ſtomach of this fiſh as a gizzard, but as a true ſtomach.

* Viz. Take ſome gillaroo-trout, male and female, and put them into water in which there are no trout, to ſee if they continue the ſame.

† I chuſe to give this name to the pancreas from its appearance.

SOME OBSERVATIONS ON DIGESTION.

THE Paper which I formerly preſented to the Royal Society, "On the Stomach itſelf being digeſted after Death", was publiſhed in 1772, in the 62nd volume of the Philoſophical Tranſactions, and has attracted the attention of Spallanzani, and others. In the courſe of theſe my obſervations, I ſhall make ſome remarks upon the experiments and opinions of theſe gentlemen; compare them with thoſe of Reaumur, and having given ſome general facts of my own upon digeſtion, ſhall conclude by adding a copy of the abovementioned Paper, with the hope that others will take up the ſubject in a more enlarged point of view, and proſecute an inquiry which is of ſo much conſequence in the inveſtigation of the operations of the animal œconomy. I cannot, at preſent, ſpare ſufficient time to give my opinious at large on this ſubject, with all the experiments and obſervations I have made upon it; but as ſoon as I have leiſure I ſhall lay them before the public.

To diſcover new parts, has been a principal object in the reſearches of the young or practical anatomiſt; but the connection, arrangement, mode of action, and uſes of the whole, or of particular organs, have more commonly been reſerved for the conſideration of thoſe whoſe views were extended further; and whoſe powers of reaſoning had been enlarged by habits of obſervation and inquiry. Curious and ſpeculative men have likewiſe made attempts in this way, but often without being ſufficiently acquainted with the ſtructure of the parts they were about to conſider; and conſequently ill-informed reſpecting their relations and connections with one another. Not contented to reaſon from thoſe which were moſt obvious, which might have led to uſeful knowledge, they have been directed by what beſt ſuited their fancy, and have principally attempted the moſt obſcure and intricate. Generation, or the mode of continuing the

ſpecies, and digeſtion, or the means of preſerving the individual, have been with them the great objects of inquiry; yet it does not appear that they have been very ſucceſsful. Although digeſtion, as being one of the moſt important operations of the animal œconomy, and moſt obvious in its effects, ſupplies a number of facts to aſſiſt in aſcertaining its powers, little has been hitherto made out towards inveſtigating the various circumſtances under which it is performed.

The mode of dividing the food, for the increaſe of its ſurface in ſome animals, ſuggeſted one method of explaining the proceſs of digeſtion; and the ſecretion of a juice, which was ſuppoſed to have the power of converting vegetable and animal matter into a fluid proper for the purpoſes of nutrition, furniſhed another. Both theſe opinions have had their advocates; and while one party contended for a mechanical power, ſuppoſed to exiſt in the gizzard; the other had recourſe to a chymical power, and conſidered fermentation as the great agent in digeſtion: they were, however, rather ſpeculative philoſophers, than practical anatomiſts, and have frequently been miſled with reſpect to the very facts and obſervations whoſe reſult was to decide the truth of their opinions. What, for inſtance, does it explain in digeſtion, that the force of the gizzard of a turkey is found equal to four hundred and ſeventy-three pounds? Does it afford a better ſolution of our doubts, than we ſhould derive from determining the force of the mill that grinds the wheat into flour? Or, on the other hand, will the moſt correct idea of fermentation enable us to account for the various phænomena in the operation of digeſtion? But we can have no very high idea of experiments made by men, who, for want of anatomical knowledge, have not been able to purſue their reaſoning beyond the ſimple experiment itſelf.

The great object ſhould have been, an endeavour to diſcover the univerſal agent in digeſtion: for the digeſtive organ is evidently conſtructed in a different manner in different animals. The mechanical power for the diviſion of the food is not univerſal; and thoſe gentlemen who conſider this power in the gizzard as the immediate cauſe of digeſtion, forgot that the ſame effect was produced in other claſſes of animals with a different

ſtructure of ſtomach, by means of the grinding teeth. Thus while the gizzard favoured the theory of the mechanical reaſoner, that idea was again deſtroyed by the membranous ſtructure of the ſtomach in many animals; which equally ſupplied the chymiſt with arguments in favour of the proceſs of fermentation.

It is more difficult than thoſe gentlemen imagine, to acquire on this ſubject information ſufficiently accurate, to be able to explain a proceſs ſo complicated as that of digeſtion. There are in Nature's operations always two obvious extremes; and the mind of man eagerly adopts that which accords with ſome principle to which he is attached, and with which he is beſt acquainted; the intermediate connections and gradations as being leſs ſtriking, not ſo forcibly affecting a ſuperficial inquirer.

It happens unfortunately, that thoſe who from the nature of their education are beſt qualified to inveſtigate the intricacies, and improve our knowledge of the animal œconomy, are compelled to get their living by the practice of a profeſſion which is conſtant employment. The only educated men who have leiſure, are thoſe of the church, ſome of whom we frequently find commencing philoſophers and phyſiologiſts; though they have not had that kind of education which would beſt direct their purſuits. Experiments, it is true, may be made by men of this deſcription; but theſe muſt neither be much complicated, nor have any immediate relation to thoſe branches of knowledge with which they have had few opportunities of being acquainted; at beſt, they will ſeldom go further than to explain a ſingle fact. To look through a microſcope and examine the red globules of blood; to view animalculæ, and give a candid account of what they ſee, are points on which ſuch inquirers may be allowed to indulge themſelves. But it is preſumption in them to affect to reaſon of a ſcience in which they can have but a very ſuperficial knowledge; or to expect to throw light on ſubjects that they have not taken the previous ſteps to underſtand. It ſhould be remembered, that nothing in Nature ſtands alone; but that every art and ſcience has a relation to ſome othre art or ſcience; and that it requires a knowledge of thoſe others,

as far as this connection takes place, to enable us to become perfect in that which engages our particular attention.

These strictures are applicable to all those who have made experiments to explain digestion. The effect of the mechanical powers being easily understood, those who considered digestion mechanically, have in general explained them justly, as far as they applied to the gizzard; but their reasoning went no further, and they supposed these effects to be digestion. Those again who took it up chymically being little acquainted with chymistry, and totally ignorant of the principles of the animal œconomy, have erroneously explained the operations of the animal machine as subject to the laws of chymistry.

The first inquirers into digestion, struck only by the extremes of structure, the gizzard, and membranous stomach, paid no regard to the gradations leading from the one to the other; which, if properly examined, would have materially assisted them to explain the functions of the stomach.

Vallisneri, considering the power of the gizzard, in one view only, imagined it would be as liable to be affected by the mechanical powers necessary for digestion, as the grain which was to be digested; therefore supposed the existence of a solvent. But though Vallisneri is intitled to no merit from this idea, as the premises are false; yet this opinion of his set Reaumur to work, and has been the means of bringing several curious facts to light. The experiments of Reaumur were first made with a view to confute that opinion; and therefore birds having gizzards, were adapted to his purpose. In this pursuit he only attended to such parts of the experiments as best accorded with his own opinion; yet carefully guarded against every possible accident that might affect their accuracy. Had trituration been the immediate cause of digestion, his experiments on the gizzards of birds were unnecessary; since it would have been sufficient to have examined the food after it had been masticated by the teeth of animals who have grinders; the teeth and gizzard answering one and the same purpose: but the circumstance of animals who masticate their food

in their mouth, having alſo a ſtomach, ſhould have taught, that there was ſomething more in digeſtion than trituration.

Reaumur's firſt experiments were made to aſcertain the ſtrength of the gizzard, with its effects; to prove that ſharp cutting ſubſtances, when ſwallowed, in no way injured its internal coat; and that the common food of the bird was not diſſolved when guarded againſt its action. Yet after all theſe proofs he ſeems to doubt, and ſays, "are we to conclude that grinding alone is ſufficient to convert the grain and other aliment into a matter proper for the nutrition of the animal, without undergoing any other preparation? Several reaſons ſeem to oppoſe this: trituration alone might reduce the grain into a flour; but flour alone is not chyle." "From the ſmell of the aliment (taken from the gizzards of birds) are we not led to conclude that it undergoes a fermentation? This ſmell may be ſaid to ariſe from the liquor with which the aliment is mixed; but is it likely that juices do not diſpoſe to fermentation ſubſtances in which it is ſo eaſily excited? Fruit and flour, made into a paſte, require little more than heat to make them ferment." From theſe experiments made with a view to prove that digeſtion is carried on by trituration, Reaumur was led to ſuppoſe a ſolvent. But as there are ſome birds whoſe ſtomachs do not ſeem ſufficiently ſtrong to have the power of trituration, he ſelected the buzzard, as being of that kind, and the fitteſt, for the ſubject of his experiments, from the circumſtance of its throwing up whatever is ſolid and indigeſtible; therefore, without killing the bird, he could know the reſult, and repeat the experiment as often as he thought neceſſary.

From the ſtomach in the buzzard being incapable of trituration, he concluded that a ſolvent was neceſſary for digeſtion; but to preclude all mechanical effects of the ſtomach, in his experiments, he employed tin tubes filled with meat, which, after the tubes had remained twenty-four hours in the ſtomach of the buzzard, was reduced to three-fourths of its ſize, was like threads, and was neither putrid, ſour, nor volatile, but inſipid. On this effect he made his remarks, which are very pertinent. In another experiment which was ſtill more accurate and concluſive, he was convinced of the action of a ſolvent. He then tried the ſoft bones of

young animals, and found they were digeſted; and that though the hard bones were not acted on ſo readily; yet by returning the ſame bones ſeveral times into the ſtomach, they were digeſted at laſt.

Reaumur was next anxious to know, if ſuch birds as were intended by Nature to live upon meat, could alſo digeſt vegetables; but the reſult was not ſo ſatisfactory. He gave bread to his buzzard, which upon being returned had the appearance of having been chewed. He next tried a piece of ripe pear; which, after having been twenty-four hours in the ſtomach had loſt ſome of its weight, and had the appearance of being boiled or baked; and thence he concludes that its powers are too weak to digeſt vegetables ſo as to nouriſh the animal.

To aſcertain the nature of the liquor which had ſuch powers, he taſted the jelly to which the meat and bone had been reduced, ſuppoſing that it muſt be well impregnated with this fluid; but he could only diſtinguiſh a bitter or a ſaltiſh taſte. To have an opportunity of more certainly determining the nature of this ſolvent, he made his buzzard ſwallow ſmall tubes filled with ſponge, which imbibed fifty grains of this liquor, having the ſame taſte as the jelly, and changing blue paper to a red. He tried the effects of this liquor on meat out of the body, with comparative experiments in water; and after twenty-four hours, the meat in the water was become putrid; but that in the liquor from the ſtomach was only ſoftened, not diſſolved. To ſee how far the analogy held good in membranous ſtomachs, he gave two bones to a dog, which being killed, after twenty-ſix hours, they were found leſſened in ſize, and become as ſoft as horn. He found that the ſtomach of the dog did not alter the ſhape of any of his tubes.

He conveyed graſs and hay, encloſed in tubes, into the ſtomachs of ruminating animals, which ſubſtances were not digeſted, but appeared as if macerated.

Let us enumerate the experiments and facts made out by Reaumur.

The gizzard was not hurt by acting upon glaſs, which it ground to a powder.

The ſtomach, or gizzard, had hardly any viſible motion.

The force of the gizzard was aſcertained.

The ſize of the ſtones found in the gizzard was in proportion to the ſize of the bird.

The ſtomach of a buzzard digeſted bone; from which he concluded the gaſtric juice had a ſolvent power; but it did not digeſt bread, altho' it acted in a ſlight degree on fruit.

He made experiments with the gaſtric juice.

The juice in the ruminating animals ſtomachs produced no effect on hay or graſs, when incloſed in tubes.

Reaumur's experiments, although not complete, paved the way for future inveſtigation; and Spallanzani proceeding on the ſame ground, has not only confirmed them by his own, but has eſtabliſhed ſeveral points not completely made out by Reaumur; for in ſome inſtances Reaumur gave up the point too ſoon, eſpecially in the experiments reſpecting the buzzard's power of digeſting vegetables. Reaumur not poſſeſſing general knowledge ſufficient to direct him in his purſuits, was neceſſarily confined to what he was moſt maſter of, the mere making experiments. Being neither an anatomiſt, nor a phyſiologiſt, he has not been perfectly juſt in his deſcription of parts; having conſidered the crop and the œſophagus leading from it to the gizzard, as two diſtinct ſtomachs: but this however is only to be ſet down as a piece of anatomical ignorance, not affecting the ſubject in the leaſt. Spallanzani is alſo deficient in his anatomical knowledge; yet it muſt be owned, that his experiments, as far as they go, are in themſelves concluſive; but like all mere experiment-makers, he is not ſatisfied even with thoſe which are clear and deciſive, but multiplies them moſt unneceſſarily, without varying them to elucidate other and eſſential parts of the ſame ſubject. I think we may ſet it down as an axiom, that experiments ſhould not be often repeated, which tend merely to eſtabliſh a principle already known and admitted; but that the next ſtep ſhould be, the application of that principle to uſeful purpoſes. If Spallanzani had employed half his time in this way, and had conſidered digeſtion under all the various ſtates of the body and ſtomach, with all the

varieties of food, both natural and artificial, he had employed his time much better than in making experiments without end.

The food of animals in general being compoſed either of vegetables, animals, or both, and a ſolvent admitted as an agent in digeſtion; it only remained to prove, that the effect of the proceſs of digeſtion was to produce from theſe various ſubſtances an animal matter, ſimilar in all animals who live on ſuch ſubſtances. But the application of principles requires more than ſimply the knowledge of the principle itſelf; and therefore thoſe who cannot reaſon from analogy, or draw general concluſions from a few convincing facts; and who require to have every relative concluſion or inference proved by an experiment, muſt be pleaſed with Spallanzani; but he muſt tire even thoſe whom he informs, and much more thoſe who read his works in expectation of ſomething new.

To make comparative experiments upon the digeſtive power, the different animals, deſtined for that purpoſe, ſhould be under ſimilar circumſtances as far as relates to digeſtion; they ſhould be equal in age, for the growing eat more than the full-grown, and of courſe digeſt faſter; which point therefore can be beſt aſcertained by ſelecting thoſe in each claſs of animals which have attained their full growth. They ſhould be equal in fatneſs, for this makes a very material difference in the powers of digeſtion in the ſame animal; and they ſhould be equal in health, a circumſtance which of all others, probably makes the greateſt difference in the powers of the ſtomach. In comparing animals of the ſame claſs, the atmoſphere ſhould likewiſe be of the ſame temperature; for the different claſſes of animals are variouſly affected by the ſame degree of heat. Experiments made upon ſnakes and lizards in the winter, will differ greatly from thoſe made in the ſummer, while ſimilar experiments made on dogs will have nearly the ſame reſult in both ſeaſons. Nor will the powers of the ſtomach be found always equal in the ſame claſs. Sleeping animals, of the quadruped kind, as hedge-hogs, do not digeſt in the winter, but in the ſummer only; therefore the concluſions to be drawn from experiments made reſpecting the digeſtive powers in the one, are not at all applicable to thoſe made in the other ſeaſon.

Spallanzani obſerved that the ſnake digeſted food faſter in June, when the heat was at 82° and 83°, than in April, when it was only 60°; from whence he concludes, that heat aſſiſts digeſtion; but this heat is not the immediate, but the remote cauſe of the increaſed power; heat having produced in the animal greater neceſſity for nouriſhment, and of courſe greater powers, gaſtric juice was therefore ſecreted faſter or in greater quantity.

As a proof that heat does not act as an immediate, but only as a remote cauſe in aſſiſting digeſtion, I ſhall mention the effect it produced upon a hedge-hog, the ſubject of Mr. Jenner's third experiment on the heat of that animal, related in the former part of this work*.

" The hedge-hog, while the heat of the ſtomach was at 30°, had neither deſire for food, nor power of digeſting it; but when increaſed by inflammation in the abdomen to 93°, the animal ſeized a toad which happened to be in the room; and upon being offered ſome bread and milk, it immediately eat it. The heat rouſed up the actions of the animal œconomy; and the parts being unable to carry on theſe actions without being ſupplied with nouriſhment, the ſtomach was ſtimulated to digeſt, to afford them that ſupply."

Spallanzani alſo mentions the ſlowneſs of digeſtion in ſerpents, and quotes Bomare, who gives an account of a ſerpent at Martinico, in whoſe ſtomach a chicken had remained for three months without being completely digeſted, the feathers ſtill adhering to the ſkin†. The truth of this fact I ſhould very much doubt, eſpecially in ſo warm a climate as that of Martinico; where I muſt ſuppoſe the digeſtive powers to be conſtantly required, unleſs there is in Martinico, as in colder climates, a torpid ſeaſon, where the act of digeſtion is not neceſſary; but in that caſe the ſerpent would not have ſwallowed the chicken. At Belliſle, in the beginning of the winter 1761-2, I conveyed worms, and pieces of meat, down the throats of lizards when they were going into winter quarters, keeping them afterwards in a cool place. On opening them at different

* Vide page 112.

† Bomare Dict. d' Hiſtoire Nat.

periods, I always found the ſubſtances which I had introduced entire, and without any alteration: ſometimes they were in the ſtomach; at other times they had paſſed into the inteſtine; and ſome of the lizards that were preſerved alive, voided them towards the ſpring, with but very little alteration in their ſtructure. So that digeſtion is regulated by the other actions of the body: warmth requires action ſuitable to that warmth; the body requires nouriſhment ſuitable to that action; and the ſtomach being called upon performs the office of digeſtion.

Nothing can ſhow more clearly that the ſecretion of the gaſtric juice is increaſed in proportion to the call for nouriſhment in the body, than what happened to Admiral Byron, Captains Cheap and Hamilton, when ſhipwrecked on the Weſt Coaſt of South-America; who, after ſuffering months of hunger and fatigue, were reduced to ſkin and bone; yet when they came to good living, Byron thus expreſſes himſelf*; "He ordered, (viz. the Governor) a table to be ſpread for us, with cold ham and fowls, which only we three ſat down to, and in a ſhort time diſpatched more than ten men with common appetites would have done. It is amazing that our eating to that exceſs we had done, from the time we firſt got among theſe kind Indians, had not killed us; we were never ſatisfied, and uſed to take all opportunities, for ſome months after, of filling our pockets when we were not ſeen, that we might get up two or three times in the night to cram ourſelves. Captain Cheap uſed to declare that he was quite aſhamed of himſelf."

Spallanzani has made ſeveral attempts to prove what few will ſubſcribe to; that ſtones in the gizzards of birds are of no uſe towards the breaking or grinding down the grain; and that they are picked up without deſign. Theſe ſtones have long been ſuppoſed to anſwer the purpoſes of trituration; and have been conſidered as affording aſſiſtance to the ſtomach, in the manner of teeth, and of courſe as being neceſſary to the act of digeſtion. Spallanzani combats this opinion; but as ſtones are univerſally found in gizzards, and it was neceſſary to account for the mode of there being conveyed there, he attributes it to chance. But we find that

* Page 181.

the gizzards which have moſt occaſion for them, and are moſt able to uſe them, are likewiſe beſt ſupplied with them: to corroborate which facts, may be added what we obſerved before, that in the larger gizzards are found the largeſt pebbles. In a turkey, two hundred were found; in a gooſe, a thouſand; which could not depend entirely upon chance. In trying whether the ſtones were of ſervice, Spallanzani introduced tubes, needles, and lancets into gizzards in which there were but very few ſtones, yet found them broken. In this experiment theſe ſubſtances had been forty-eight hours in the gizzards; whereas in the former experiments, with the ſame kind of tubes, thirty-ſix hours was the longeſt time; in another, eighteen hours; and in another, the breaking of them was begun in leſs than two hours; therefore the experiments were not perfectly fair, as the times were not equal. What he thinks the moſt concluſive is, that where he had taken care there ſhould be no ſtones, the hard indigeſtible ſubſtances were acted upon much in the ſame way as when there were ſtones; but in this experiment he does not give the time, which is very accurately ſtated in moſt of the others.

He diſcovered that the inner ſurface of the ſtomach was not hurt by ſuch ſubſtances; and indeed it is ſcarcely poſſible for the inner coat of the ſtomach of a fowl to be pierced by ſuch as are even ſharp pointed, the quantity of its motion being ſo inconſiderable, as hardly to make them paſs through its inner coat. But the principal cauſe of their being harmleſs, ariſes from the motion being lateral, and not preſſing perpendicularly to the axis, one ſurface ſliding in a contrary direction to the other; and that not in a ſtraight, but in a circular direction, as will be explained hereafter.

In conſidering the ſtrength of the gizzard, and its probable effects when compared with the human ſtomach, it muſt appear that the gizzard is in itſelf very fit for trituration; we are not however to conclude that ſtones are entirely uſeleſs; for if we compare the ſtrength of the muſcles of the jaws of animals who maſticate their food, with thoſe of birds who do not, we ſhall ſay that the parts are well calculated for the purpoſe of maſtication; yet we are not from thence to infer that the

teeth in ſuch jaws are uſeleſs, even although we have proof that the gums do the buſineſs when the teeth are gone. If ſtones are of uſe, which we may reaſonably conclude they are, birds have an advantage over animals having teeth, ſo far as ſtones are always to be found, while the teeth are not renewed. Spallanzani concludes, " That we have at length a deciſion of the famous queſtion concerning the uſe of theſe pebbles, ſo long agitated by authors; it appearing that they are not at all neceſſary for the trituration of the firmeſt food, &c." but ſays, " He will, however, not deny that when put in motion by the gaſtric muſcles, they are capable of producing ſome effects on the contents of the ſtomach." Now if we conſtantly find in an organ ſubſtances which can only be ſubſervient to the functions of that organ, ſhould we deny them to be of any uſe, becauſe the part can to a certain degree do its office without them?

To account for pebbles being found in the gizzard, Spallanzani ſuppoſes the birds to have picked them up by chance, or not to have diſtinguiſhed between their food and theſe ſtones. But it appears ſingular, that only thoſe which have gizzards ſhould be ſo ſtupid; and he owns, that Redi and himſelf found that birds died of hunger, yet without having picked up more ſtones than uſual; which we might ſuppoſe they would have done if they had not had a choice, or could not have diſtinguiſhed ſtones from the grain on which they feed.

The ſtones aſſiſt in breaking the grain, and by ſeparating its parts in the beginning of the proceſs, and afterwards by rubbing off the ſurface already digeſted, allow the gaſtric juice to come more completely in contact with the whole.

It has been ſaid, that the motion of the gizzard is ſo ſmall as hardly to be obſervable; and that it cannot be felt by the hand. But as its cavity is very ſmall, and muſt be capable of adapting itſelf to the quantity it contains, (or it could not poſſibly grind) much motion is not neceſſary for the purpoſes of trituration: a ſwelling and collapſing, like the motion of the heart, would have no effect. The extent of motion in grindſtones need not be the tenth of an inch, if their motion is alternate and in contrary directions. But although the motion of the gizzard is hardly viſible,

yet we may be made very ſenſible of its action by putting the ear to the ſides of a fowl while it is grinding its food, when the ſtones can be heard moving upon one another.

It may be remarked, that the motion of the whole inteſtinal canal, from the fauces to the anus, is naturally ſo ſlow, as not to be excited into quick actions. The food paſſes ſlowly along the œſophagus; and in a man, fluids which might be expected to act even by their own gravity, deſcend but ſlowly: yet I think we may be certain that the œſophagus has always a regular contraction; and that the lower parts muſt relax in progreſſion, as it contracts above; ſo that no poſition of the body makes any difference in this action.

Upon expoſing the ſtomach in living animals, it does not appear much agitated or affected, even by being handled or otherwiſe irritated. The ſame thing may be obſerved in the whole track of inteſtines: and we find that when the fæces are expelled by the action of the gut alone, that the expulſion is ſlow; the ſtomach and rectum, however, can be emptied at once; but that is done by the abdominal and other muſcles. We know that the action of vomiting is performed entirely by the diaphragm and abdominal muſcles; and we know that by the ſame action the contents of the rectum can be expelled. Neither is any other power required to empty the ſtomach in vomiting, theſe muſcles being often capable of forcing the bowels themſelves out of the abdomen, and of producing a rupture. It is not neceſſary the ſtomach itſelf ſhould act violently to produce an evacuation of its contents; nor is it even neceſſary it ſhould act at all: for the lungs themſelves do not act in the leaſt when any extraneous matter is to be thrown up; and coughing is to the lungs, what vomiting is to the ſtomach. The muſcles of reſpiration are the active parts in emptying the lungs, and can act both naturally and preternaturally. The muſcles of the thorax and abdomen do not act naturally on the contents of the abdomen, but often act preternaturally, producing an evacuation from its viſcera.

There is this difference in the action of the parts in coughing and vomiting: the cough is performed by the proper muſcles of reſpiration,

which are thoſe of expanſion, ſupported by the abdominal, while the diaphragm is paſſive.

Vomiting is performed by the abdominal muſcles and diaphragm; while thoſe of inſpiration are ſupporting this action.

In coughing, the ribs are ſuddenly depreſſed, which diminiſh the capacity of the thorax; and that the diaphragm may not be allowed to ſink down and increaſe the capacity of the thorax, which would counteract the depreſſors of the ribs; the abdominal muſcles at the ſame time act, which ſupports the diaphragm in its place, and probably may by this action aſſiſt in bringing down the ribs. To give as much force to this action as poſſible, the glottis is ſhut till the action is begun, and then the glottis opens inſtantaneouſly, which obliges the depreſſors of the ribs to begin the effort with their full action.

The proper muſcles of inſpiration do not tire ſo ſoon in this action as the abdominal; for in violent coughing the muſcles of the abdomen become ſore.

In vomiting, theſe actions are reverſed. The muſcles of the cavity of the abdomen act, in which is to be included the diaphragm; ſo that the capacity of the abdomen is leſſened, and the action of the diaphragm rather raiſes the ribs; and there is alſo an attempt to raiſe them by their proper muſcles, to make a kind of vacuum in the thorax, that the œſophagus may be rather opened than ſhut, while the glottis is ſhut ſo as to let no air enter the lungs. The muſcles of the throat and fauces act to dilate the fauces, which is eaſily felt by the hand, making there a vacuum, or what is commonly called a ſuction; ſo that when all theſe actions take place together, the ſtomach is immediately emptied.

In violent coughing, we find that a kind of mixed action takes place; for although the diaphragm has not acted, yet the ſtomach is ſo much ſqueezed as to diſcharge its contents; and it affects the diaphragm, which is often thrown into action, and brings on vomiting at the ſame time; therefore violent coughing palls the ſtomach.

There is reaſon to believe that the natural motion in all ſtomachs is regular; and I am more inclined to be of this opinion, from what takes

place in the ſtomach of animals who are covered with hair, and who lick their own bodies; and of ſuch as feed on whole animals, who are likewiſe covered with hair. In the ſtomach of the calf, for inſtance, who licks his ſkin with his tongue, and ſwallows whatever is attached to the rough ſurface of that organ, balls of hair are often found; and on examining their ſurface, the hairs in each hemiſphere ſeem to ariſe from a centre, and to take the ſame direction, which is circular, correſponding to what would appear to be the axis of this motion, and reſembling what we ſee in different parts of the ſkin of animals whoſe hair takes different turns. This regularity in the direction of the hair, in ſuch balls, could not be produced if there was not a regular motion in the ſtomach. This motion is alſo proved in the dog; for I have ſeen a ball of this kind, that had been thrown up from a dog's ſtomach, where the ſame regularity in the turns of the hair was very evident and complete. The ſame motion ſeems alſo to take place in the bird kind; and of this the cuckoo is an example, which, in certain ſeaſons living on caterpillars, ſome of whom have hairs of a conſiderable length on their bodies, the ends of theſe are found ſticking in the inner horny coat of the ſtomach or gizzard, while the hairs themſelves are laid flat on its ſurface; not in every direction, which would be the caſe if there was no regular motion, but all one way, ariſing from a central point placed in the middle of the horny part; and the appearance on the ſurface of both ſides of the gizzard evidently correſponding. Theſe two facts prove, in my opinion, a regular circular motion taking place in the gizzard and membranous ſtomach; and therefore, moſt probably, ſomething ſimilar is carried on in ſtomachs of all the various kinds. Indeed this motion in the ſtomach is ſo conſiderable, that when there is no horny defence, we find the coats ſometimes pierced by hard pointed ſubſtances. Thus the cows who feed on the graſs of bleaching-grounds have their ſtomachs, eſpecially the ſecond, ſtuck full of pins: and fiſh who prey upon, and ſwallow other fiſh entire, often have their ſtomachs pierced by the bones.

Spallanzani calls the inner coat, cartilagenous; whereas, in fact, it is a horny ſubſtance, forming an inner cuticle, but differing in ſome reſpects

from the common cuticle; this horny ſubſtance not only differs in ſtructure from the common cuticle, but in its attachment, from cuticle, nails and hoofs. The cutis, where it is covered by ſuch ſubſtances, has a vaſt number of villi on its ſurface, which paſs into correſponding perforations in the cuticle; from this ſtructure of parts, when the cuticle, nails, or hoofs are ſeparated, their inner ſurface appears to be full of ſmall perforations; and the cutis from which they have been removed is villous; and theſe villi are more numerous in ſome parts than in others, where the ſenſe of touch is required to be delicate or acute. But the inner lining of the gizzard is juſt the reverſe; that ſurface of the horny ſubſtance which is in contact with the gizzard being villous; and when ſeparated, the inner ſurface of the gizzard appearing perforated. Theſe villi are either the laſt formed parts of this horny ſubſtance, or are the fibres of which the horny coat is compoſed. It is probable, that this horny ſubſtance takes the form of villi that it may be more firmly connected with the gizzard, in which acute ſenſation is not required.

I may remark here, that the experiments made on the digeſtion of ruminating animals have been very deficient, ariſing from this proceſs in them being more complicated than in the ſtomachs of other animals; and requiring attention to be paid to certain circumſtances, which cannot take place in ſtomachs of only one cavity.

The circumſtance mentioned by Spallanzani, of ruminating animals voiding the tubes by the anus, ſhows that the whole food is not neceſſarily returned into the mouth to be chewed a ſecond time; for if it were, the tubes would certainly come up likewiſe, and would as certainly be thrown out of their mouths as improper to be chewed, a circumſtance which often really happened. But it was hardly neceſſary to make experiments to aſcertain whether ruminating animals digeſted meat, when we know that in ſome cold countries, the cattle are fed on dried fiſh; and moſt animals eat their own ſecundines: indeed the circumſtance of animals living upon both animal and vegetable food might have taught us, that the mode of digeſting both (whatever it is) was the ſame; therefore all that was wanted muſt have been to diſcover that mode; except

we could abſurdly conceive, that two different modes might take place in the ſame ſtomach at the ſame time.

Spallanzani gives the opinion of authors reſpecting digeſtion; and ſo anxious is he to combat the idea of its being fermentation, that he will hardly allow that fermentation ever takes place in the ſtomach. That fermentation can go on in the ſtomach, there is no doubt; but when that happens, it ariſes from the powers of digeſtion being defective. Milk, vegetables of all kinds, wine, and whatever has ſugar in its compoſition, become much ſooner ſour in ſome ſtomachs, than they would, if left to undergo a ſpontaneous change out of the body: and even ſpirits, in particular ſtomachs, almoſt immediately degenerate into a very ſtrong acid. I am inclined to ſuppoſe, that it is the ſugar which is converted into ſpirit, and the ſpirit into acid; conſequently, a glaſs of brandy, from being much ſtronger, becauſe leſs diluted, moſt probably contains as much matter, likely to become acid, as half a pint of wine. In other ſubſtances, beſides thoſe mentioned above, the fermentative proceſs (unleſs prevented by that of digeſtion) appears to begin ſooner in the ſtomach than out of the body. All oily ſubſtances, particularly butter, very ſoon become rancid after being taken into the ſtomach; and this rancidity is the effect of the firſt proceſs of the fermentation of oil. Mr. Sieffert has been able to reſtore rancid oils to their original ſweetneſs, by adding to them their due quantity of fixed air*; the loſs of which I conſider as the firſt proceſs in this fermentation, ſimilar to what happens in the fermentation of animal and vegetable ſubſtances.

Animal food does not ſo readily ferment in the ſtomach, when combined with vegetables, as when it is not; for the vegetables running more quickly into fermentation, preſerve the meat from putrefaction. Put a piece of meat and ſome ſugar, or bread, into water, and let them ſtand in a warm place, the bread and ſugar will begin to ferment, the water will become ſour, and the meat be preſerved; but the acid becoming weaker, as the fermentation advances towards the putrefactive, the meat at laſt

* Phyſical and chymical Eſſays, by Sir Tobern Bergman.

begins to acquire the ſame putrid diſpoſition*. Yet this laſt part of the proceſs cannot, I think, take place in the ſtomach, as a ſucceſſion of acids will be formed, by which the meat will be preſerved ſweet till it is digeſted: the formation of this acid in the ſtomach, moſt probably, not preventing the digeſtion of thoſe ſubſtances which are incapable of being rendered acid.

Bread allowed to remain in the ſtomach of a dog for eight hours, is ſo much changed, that it will not run into the vinous fermentation; but when taken out and kept in a warm place, becomes putrid: its putrefaction, however, is not ſo quick as a ſolution of meat that has been in the ſtomach for the ſame length of time. Similar effects are produced when milk and bread are the food adminiſtered; and perhaps the gaſtric juice, when in ſufficient quantity, will always prevent the vinous fermentation.

Spallanzani's next trials were to determine, whether the gaſtric juice had the power of recovering meat already putrid: a fact which might have been proved by one experiment. For if very putrid meat is given to a dog, and the dog killed after ſome time, the meet will be found ſweet, and all putrefaction at an end. Therefore his allowing freſh meat to continue a longer or ſhorter time in the ſtomach was immaterial, as it could not become putrid.

It appears from the above facts, that the ſtomach has not ſo much power in preventing the acetous fermentation in vegetables, as in correcting the putrefactive diſpoſition in animal ſubſtances. For although this cannot be certainly known in thoſe who eat both animal and vegetable food; yet it does not appear that the putrefaction of animal ſubſtances, where nothing elſe is eaten, takes place ſo quickly in the ſtomach, as the change which is produced in vegetables; the acetous diſpoſition is therefore either ſtronger than the putrefactive, or it more readily takes place: and indeed the living body ſhews this ſufficiently; for we very often find an acid thrown up, but ſeldom or ever any thing putrefactive.

It may be admitted as an axiom, that two proceſſes cannot go on at the ſame time, in the ſame part, of any ſubſtance; therefore neither vegetable

* Of this Sir John Pringle was not aware in making his experiments on this ſubject.

nor animal ſubſtances can undergo their ſpontaneous changes while in the act of being digeſted; it being a proceſs ſuperior in power to that of fermentation. But if the digeſtive power is not perfect, then the vinous and acetous fermentation will take place in the vegetable; and the putrefactive in the food of thoſe animals which live wholly on fleſh; although in the laſt I imagine but very ſeldom. The gaſtric juice, therefore, preſerves vegetables from running into fermentation, and animal ſubſtances from putrefaction; not from any antiſeptic quality in the juice, but by making them go through another proceſs, preventing the ſpontaneous change from taking place. In the greater number of ſtomachs there is an acid, even although the animal has lived upon meat for many weeks; but as this is not always the caſe, we muſt ſuppoſe it is only formed occaſionally. Whether the ſtomach has a power of immediately ſecreting this acid, or firſt ſecretes a ſugar which afterwards becomes acid, is not eaſily aſcertained: but I ſhould be inclined to ſuppoſe, from analogy, the laſt to be the caſe; animals in health ſeeming to have the power of ſecreting ſugar; for we find it in the milk; and ſometimes in the urine, in conſequence of diſeaſe. Acid ſometimes prevails in the ſtomach to ſo great a degree, as to become a diſeaſe, attended with very diſagreeable ſymptoms; the ſtomach converting all ſubſtances which have a tendency to become acid, into that form. To aſcertain whether there was an acid naturally in the ſtomach, the moſt ſatisfactory mode was to examine the contents before the birth, when the digeſtive organs are perfect, and when no acid could have been produced by diſeaſe, or any thing that had been ſwallowed: accordingly in the ſlink-calf, near the full time, there was no acid found in the ſtomach; although the contents had the ſame coagulating powers with thoſe of animals who have ſucked.

As we find ſtomachs poſſeſſed of a power of diſſolving the whole ſubſtance of a bone, it is reaſonable to ſuppoſe that its earth is deſtroyed by the acid in the ſtomach.

The ſtomach appears not only to be capable of generating an acid, but alſo to have the power of producing air; which laſt effect, I believe, ariſes from diſeaſe. It is not eaſy to account for the formation of this air; yet

as the ſtomach is a reſervoir for ſubſtances diſpoſed to ferment, it might reaſonably be ſuppoſed to ariſe from the food going into that proceſs. But this, in my opinion, will not account for the vaſt quantity of air frequently thrown up from the ſtomach, even where food has not been ſwallowed for a conſiderable time, and where digeſtion appeared to have been completed. For we muſt conclude this proceſs to have been completed, if the food was not found to have diſagreed with either ſtomach or bowels, and that the ſtools were good. When the gout falls on the ſtomach, the quantity of air thrown up is often immenſe; and the ſame thing may be obſerved in ſome caſes commonly called nervous; yet the proceſs of digeſtion will not account for this formation of air; as no air is to be found in healthy ſtomachs*; neither is it to be accounted for from a defect in digeſtion, as that would probably be productive of worſe conſequences.

I am inclined to believe that the ſtomach has a power of forming air, or letting it looſe, from the blood, by a kind of ſecretion. We cannot, however, bring any abſolute proof of this taking place in the ſtomach, as it may in all caſes be referred to a defect in digeſtion: but we have inſtances of air being found in other cavities, where no ſecondary cauſe can be aſſigned. I have been informed of perſons who have had air in the uterus or vagina, without having been ſenſible of it, but by its eſcaping from them without their being able to prevent it; and who, from this circumſtance, have been kept in conſtant alarm leaſt it ſhould make a noiſe in its paſſage, having no power to retard it, as when it is contained in the rectum. This fact being ſo extraordinary, made me ſomewhat incredulous; but rendered me more inquiſitive, in the hope of being enabled to aſcertain and account for it: and thoſe of whom I have been led to inquire, have always made the natural diſtinction, between air paſſing from the vagina, and by the anus; that from the anus they feel and can retain; but that in the vagina they cannot; nor are they aware of it till it paſſes. A woman, whom I attended with the late Sir John Pringle,

* In all my experiments on digeſtion, in dogs, I have never been able to detect any air in the cavity of the ſtomach.

informed us of this fact; but mentioned it only as a disagreeable thing. I was anxious to determine, if there were any communication between the vagina and rectum, and was allowed to examine, but discovered nothing uncommon in the structure of these parts. She died some time after; and being permitted to open the body, I found no disease either in the vagina or uterus. Since that time I have had opportunities of inquiring of a number of women, concerning this circumstance, and by three or four have been informed of the same fact, with all the circumstances abovementioned: how far they are to be relied upon I will not pretend to determine. I have likewise found air in the cellular-membrane, in gun-shot wounds, that had passed some way under the skin, without being able to account for its being there by any mechanical effect of the ball.

That air is either formed from the blood, or let loose by some action of the vessels, both naturally and from disease, is an undeniable fact. We find air formed in some fishes, to answer natural purposes; for in those whose air-bladders do not communicate externally (many of which there are) we must suppose it to have been formed there. We also find it in animals after death; and I have a piece of the intestine of a hog which has a number of air-bladders upon it*. Mr. Cavendish was so kind as to examine this air, and he found "it contained a little fixed air; and the remainder not at all inflammable, and almost completely phlogisticated." I have often seen such vesicles on the edges of the lungs; but these may be supposed to have been a kind of aneurismal air-cells filled from the trachea, and are circumscribed and impervious; so that in the state we find them, they have no communication with the external air. In one instance I have discovered air in an abscess, which could not have been received from the external air; nor could it have arisen from putrefaction. The case is as follow:

A lady, about forty years of age, had been afflicted with complaints in the bladder and parts connected with it. From the symptoms, her disease was supposed by some to be the stone, though upon examination no

* Vide Plate.

ſtone was found; and ſhe had alſo an umbilical hernia, for which I had been conſulted. She grew gradually worſe; and from being luſty became a thin woman. A ſmall tumor appeared in the groin, and the ſkin over it became red, ſimilar to an abſceſs when the matter is beginning to point externally; but before her death this ſubſided. A few days before ſhe died, I was deſired to examine a ſwelling on the lower and right-ſide of the belly, extending nearly from the navel to the ſpine of the ilium on the right-ſide. It was tenſe, evidently contained air, and could be made to ſound almoſt like a drum. It had come on within a few weeks, and I was puzzled to account for it; there being clearly no connection between that tumor and the umbilical hernia. I was inclined to ſuppoſe it to be a ventral hernia, containing the cæcum and part of the colon, filled with air; but as ſhe had ſtools; as there were no ſymptoms of a ſtrangulated gut, nor any uneaſineſs in the bowels; as I could not make the air recede, but felt it as if confined to that part, I own I could form no conjecture what the caſe really was. The woman dying in a few days, I was permitted to examine the body. That I might not interfere with the tumor, or umbilical hernia, I made an opening into the abdomen on the right-ſide of the linea alba; and on examining the cavity of the abdomen, found every thing natural, except a ſmall portion of the epiploon adhering to the inſide of the navel; the parietes of the abdomen correſponding with the tumor, being in a natural ſtate. On preſſing the tumor by the hand, air was heard to make its eſcape: whether by the vagina or anus was at firſt doubtful; but on examining with more attention, it was diſcovered to come from between the labia. I next opened the tumor externally, and let out the air, which was not in the leaſt putrid, and was contained in a ſac tolerably ſmooth on its inſide, made up of compreſſed cellular-membrane; the abdominal muſcles and tendons forming the poſterior ſurface, which extended as low as the inferior edge of Poupart's ligament. The contents of the abdomen were tolerably ſound; but when I inſpected the viſcera contained in the pelvis, they were found adhering to each other; the bladder to the body of the uterus; the broad ligaments and ovaria, to the uterus; and on examining theſe adheſions, I diſcovered

a cavity between the bladder, uterus, and vagina on the right-ſide, ſomething like an abſceſs. From the right-ſide of this cavity there was a canal aſcending to the brim of the pelvis, in the courſe of the round ligament, as far as to the going out of the iliac veſſels, which it ſeemed to accompany; and this canal when it paſſed from behind Poupart's ligament, communicated with the tumour abovementioned. I then endeavoured to diſcover if there were any communication between the rectum and the abſceſs; but could find none, the gut appearing to be quite ſound. Having removed the whole contents of the pelvis, with the canal leading to Poupart's ligament, and the ligament itſelf, with ſuch of the abdominal muſcles as compoſed part of the ſac, I found both the rectum and vagina perfectly ſound. The uterus had a polypus forming on its inſide; neither the rectum nor uterus had any connection with the abſceſs; but there was a ſmall communication between the abſceſs and the bladder; that portion of the bladder which made part of the abſceſs being very much diſeaſed.

From this hiſtory of the appearances of the tumour before death, and the particular account I have given of the diſſection, the reader may be able to draw his own concluſions relative to the origin of the air. It certainly appeared to have been formed in this bag; and it was only towards the latter end of her life that it could have made its eſcape into the cavity of the bladder; for it was not poſſible to ſqueeze the air out of the tumor, when I firſt ſaw her; but juſt before death it became more flaccid. It could not be formed or let looſe in conſequence of putrefaction, for the air itſelf was free from any ſmell; and although the cavity between the vagina and bladder had on its internal ſurface the irregular ulcerated appearance of an abſceſs, yet that on the abdomen had not, was tolerably ſmooth, and had rather the appearance of having been formed in conſequence of ſome foreign matter accumulating there.

This circumſtance, of an animal having the power of forming air, or ſeparating it from the juices by a kind of ſecretion, appears at firſt view to be ſupported by the experiments of Dr. Ingen-houſz.*

* Exp^ts upon Vegetables; proving their great Power of purifying the common Air, &c.

The Dr. obſerved that, when we immerſe our bodies " in a cold or warm bath"; or, " By plunging the hand and arm even in cold water," that globules of air ſoon appear upon the ſkin: and to be certain of the air coming from the body, he took all the neceſſary precautions to prevent the external air being carried into the water along with the body, (which would certainly be a conſequence, if the body or part were immerſed quickly, or when dried.) But although his experiments ſeem to prove this opinion, yet I imagine there is a circumſtance the Dr. did not attend to at the time, which renders them very fallacious; for he did not conſider that water, for the moſt part contains a great deal of air; therefore the globules of air might as readily come from the water, as from the body; which makes it neceſſary to aſcertain, by experiment, from whence the air comes which is attached to the body when immerſed in water.

Water takes up air in proportion to its coldneſs, until it loſes the property of water and becomes ſolid: upon this principle we may account for globules of air being found attached to the ſkin, when a part of the body is immerſed in water colder than itſelf; for when we immerſe the whole body we increaſe the heat of the water, eſpecially that next to the ſkin; and if we immerſe only a part, as an arm, it being commonly in a ſmaller quantity of water, the water immediately ſurrounding it is alſo warmed. As a proof that it is the air from the water, and not from the ſurface of the body* it matters not what the ſubſtance is that is immerſed, if it is but warmer than the water: for a piece of iron heated to about 150°,

* " Count de Milly, in the Berlin Tranſactions for the year 1777, publiſhed experiments to ſhow that there is an excretion of air; or, as it is termed, 'an aerial tranſpiration,' from the whole ſurface of the human body while it remains in warm water: but Dr. Pearſon found, on repeating theſe experiments, that there was no appearance of aerial bubbles on the ſurface of the cuticle during bathing in warm water that had been previouſly boiled, ſo as to expel the air uſually mixed and united to river, and ſpring-water. The human body when immerſed in the bath at Buxton, and kept at reſt in it for ſome time, was covered with air-like bubbles; but theſe bubbles appeared in the ſame manner, on any ſolid body whatever that was placed in it. It is therefore ſuppoſed that the attraction to the human body of the air, commonly ſuſpended in water, eſpecially when heated to the temperature of a warm-water bath, has been miſtaken for an excretion of air from the cuticle."

immerſed in water about 70°, will warm the water in contact with it ſo as to make it part with its air. This effect of heat is further proved, by making another trial, with only this difference, that the iron be ten degrees colder than the water; in that caſe little or no air will be ſeparated, and of courſe no bubbles obſerved. The bubbles of air do not appear to ariſe entirely from the degree of warmth of the water; but alſo in ſome meaſure from a ſolid body being immerſed in it, that ſeems to have a power of attracting the air, whoſe affinity to the water is now weakened by heat; for ſimply heating the water to the ſame degree will not ſeparate the air, as we find that no bubbles are then produced. The power of attracting the air appears therefore, in ſome ſort, to depend upon the ſolidity of the body immerſed; at leaſt, bodies have a greater number of bubbles in proportion to their ſolidity: for upon making comparative experiments between iron, ſtone, wood, and cork, the air ſeparated from the water upon the ſurface of the iron and ſtone is in conſiderable quantity; that upon the wood very ſmall; and ſcarcely any at all upon the cork.

As theſe obſervations on the generation (or ſecretion) of air in cavities, ſeemed to have a connection with the preſent inquiry, I thought they might, properly enough, be introduced here; but I ſhall content myſelf with having mentioned the circumſtance, and purſue the ſubject of digeſtion.

To determine with abſolute certainty, in what particular portion of the canal this important proceſs of digeſtion is performed, is perhaps impoſſible; but there is the greateſt reaſon to believe that it is principally carried on in the ſtomach, with a little variation in different animals. We may venture to affirm that, it does not at all take place in the long and contracted œſophagus of the quadruped; the ſecretion of that part being a ſlimy mucus, poſſeſſed of no power ſimilar to that of the gaſtric juice; but only intended to facilitate the paſſage of the food into the ſtomach.

Neither has the mucus ſecreted in certain parts of the œſophagus of birds, as in the crop of thoſe who have one, any digeſtive power; while, on the contrary, we find the lower end, which is extremely glandular, to

be capable of ſecreting a juice with all the properties of the gaſtric; and that paſſing into the cavity of the ſtomach, becomes a ſubſtitute in this claſs of animals for the deficiency of the ſecretion of the ſtomach itſelf; which in ſome is lined with a horny ſubſtance, and in others with a cuticle. Even in birds, the ſeat of digeſtion is chiefly in the ſtomach; the juice ſecreted in the lower part of the œſophagus paſſing into that cavity; and the mucus ſecreted by the other parts of the œſophagus, as in the crop of thoſe who have one, has no ſuch power. But if any digeſtible ſubſtance ſhould be retained in the œſophagus, as may happen in many of thoſe who ſwallow whole animals, digeſtion may even go on in its inferior portion. In the gull and heron, which take down ſnakes and fiſh entire, the tails may remain in the œſophagus till the head is digeſted in the ſtomach; and in ſuch caſes the tail itſelf may be acted upon in that ſituation.

As a further proof that digeſtion is carried on principally in the ſtomach, let us obſerve what happens to the yolk of an egg in the bird newly hatched. The yolk is not in the leaſt conſumed in the time of incubation; but appears to be reſerved for the nouriſhment of the chick, between the time of hatching, and its either being ſupplied with food by its parents, or being able to procure it for itſelf: for we find, that although the yolk paſſes into the gut at ſome diſtance from the ſtomach, yet it is carried up to the ſtomach to be digeſted; and I have even ſeen it in the crop, being retained there till wanted.

In thoſe animals whoſe ſtomach conſiſts of ſeveral cavities, the preciſe place where digeſtion is carried on has not been aſcertained. I think, however, that in the ruminating claſs, who have four cavities, it may be ſet down as a fact, that digeſtion goes on in the fourth; which is beſt proved by feeding the animal with a ſubſtance that does not require any kind of preparation for digeſtion, ſuch as milk. If a calf be killed about half an hour after it has ſucked its mother, we ſhall find the whole milk in the fourth cavity firmly coagulated, and formed into a ball; while the firſt, ſecond, and third cavities, contain only ſuch food as requires maſtication, or what other preparation is neceſſary to fit it for digeſtion. Such

animals have the power of conveying the food from the œſophagus, either to the firſt or fourth cavity, according to the nature of the food; and for this purpoſe there is a groove leading directly from the œſophagus to the fourth ſtomach, which I ſuppoſe can be converted into a canal when wanted.

It is poſſible that digeſtion may likewiſe be carried on in the duodenum, eſpecially in its upper part; if either the inteſtine ſecretes the ſame juice with the ſtomach; or that ſome of the gaſtric juice, and part of the food, has paſſed into the inteſtine before it has been completely turned into chyle.

Although the ſtomach is the ſeat of digeſtion, it is not ſolely appropriated to that purpoſe; and in many animals theſe organs are not to be conſidered as only a digeſting bag or bags, but in part as a reſervoir for food. This is moſt remarkable in the ruminating animals, where the firſt ſtomach or bag is merely a reſervoir, and in this reſpect analogous to a crop. It is the ſame in the porpus, and, I believe, in moſt animals of this claſs: although it cannot be ſuppoſed that thoſe return the food who have not the power to maſticate. In ſome animals, who do not ruminate, there is not the ſame neceſſity for diſtinct pouches; the ſtomach conſiſting either of one bag, ſingly; or of ſtomach with appendages, as in the Pecari. But the whole organ is not endowed with the property of ſecreting the gaſtric juice; there being a part whoſe ſtructure is very different from that appropriated to digeſtion, and covered by a cuticle, as in the firſt, ſecond, and third ſtomach of the ruminating animals; and in the firſt ſtomach of the porpus. The pecari, the common hog, and the rat, are likewiſe inſtances of this; and the ſame circumſtance takes place, in a ſmaller degree, in the horſe. This increaſe in the cavity of the ſtomach, beyond what is neceſſary for digeſtion alone, is peculiar to the animals that take in more food than is immediately wanted, or whoſe food is of a nature which requires a certain degree of preparation prior to digeſtion. The crop of the eagle, and perhaps the ſtomach of the porpus, are of the firſt kind; the crop in the gallinaceous fowls, and the firſt ſtomach in ruminating animals, of the ſecond. It is the diſpoſition of ſuch animals to

fill these cavities; and the quantity which they are capable of containing, makes them seldomer require to be filled: it is probable, likewise, that it is the sensation excited by this fulness which gives satisfaction to the animal, and takes off the further desire for food; an effect similar to what is produced in other animals from filling the stomach itself; and these having no such provision, are longer and oftener employed in pursuit of food.

I should be apt to consider the power of the gastric juice to coagulate milk, and some other animal mucilages*, as a test of the stomach being the seat of digestion: for although milk may be coagulated by other substances, yet when found in that state in the stomach, it is probably for the purpose of digestion; milk, and many other natural substances, requiring to be coagulated before they can be digested. I have found this coagulating power in the stomach of every animal that I have examined for that purpose, from the most perfect down to reptiles: and in the appendages which I have considered as only reservoirs preparatory to digestion, (as the first stomach in the ruminating animal, and the crop in birds) I have discovered no such power. Yet it is not the digestive power which coagulates those substances; complete coagulation taking place even where digestion does not at all go on. This is evident every day in children that suck, and who have diseased stomachs; for we see them throw up the milk coagulated, and discharge it undigested by stool. A very remarkable instance occurred in a child that had lost entirely the power of digestion; yet the milk taken down came away strongly coagulated; some even as firm almost as cheese; which seems to shew, that the coagulating power is seldom wanting, although the other may.

The gastric juice is a fluid somewhat transparent, and a little saltish or brackish to the taste; but whether this is essential, or only accidental, is

* Milk is the substance commonly known to be coagulated by the gastric juice: but I find that it has also the same power over the white of an egg. Give to a dog some raw egg, and kill him half an hour after he has swallowed it, the egg will be found coagulated in his stomach, as if boiled; the crystalline humour, in the stomachs of fishes, is likewise found coagulated.

not eaſily determined. Indeed, there are very few of our ſecretions which have not ſome ſalt in them; it being found in the tears, the ſaliva, the ſecretion of the glans penis, of the glands of the urethra, and in the firſt and the laſt milk ſecreted in the udders of animals.

I am not inclined to ſuppoſe that there is any acid in the gaſtric juice as a component or eſſential part of it; although an acid is very commonly diſcovered, even when no vegetable matter has been introduced into the ſtomach*. The acid may be increaſed in ſome diſeaſes; and in others the diſpoſition to form it may be deſtroyed; which may be the reaſon why, by a kind of inſtinctive principle, many girls are fond of eating ſour fruit, and of drinking vinegar; while others, on the contrary, from a different cauſe, often eat chalk, lime, and other ſubſtances of that ſort: but the acid not being always found, it is not yet determined on what occaſions it is formed, or in what manner it is deſtroyed.

The proceſs of digeſtion differs from every other natural operation, in the change it produces on different bodies; yet is by no means fermentation, though it may ſomewhat reſemble it. For fermentation, a ſpontaneous proceſs, is that natural ſucceſſion of changes by which vegetable and animal matter is reduced to earth; therefore muſt be widely different from digeſtion, which converts both animal and vegetable ſubſtances into chyle; in the formation of which there cannot be a decompoſition, ſimilar to fermentation.

* The only trial to which I ever put the gaſtric juice, was with the ſyrup of violets, to aſcertain if it was acid; and in many of the trials the colour of the mixture was changed to red: but it is neceſſary for the accuracy of the experiment, which is to determine this fact, that the animal ſhould not be fed upon vegetables for ſome time before the trial is made, theſe being liable in ſome degree to become ſour; therefore it is hardly fair to make the experiment on the contents of the ſtomach of animals who live upon vegetables. In many trials of this kind we may be deceived, and led to ſuppoſe an alkali. For certain animal ſecretions being of a yellow caſt, when ſuch are mixed with the ſyrup of violets, the mixture is changed to a green. The truth of the experiment may, however, be known by adding a little acid; for if the green has been produced merely by a mechanical mixture, it will become immediately a ſcarlet, by being then a mixture of red and yellow: but if the ſecretion is not only of a yellow colour, but of an alkaline nature, it will alſo continue green; and by adding a little more acid than what ſaturates the alkali, the colour will then become orange.

Digeſtion is likewiſe very different from chymical ſolution, which is only an union of bodies by elective attraction. But digeſtion is an aſſimilating proceſs; and in this reſpect is ſomewhat ſimilar in its action to that excited by morbid poiſons. It is a ſpecies of generation, two ſubſtances making a third; but the curious circumſtance is its converting both vegetable and animal matter into the ſame kind of ſubſtance or compound, which no chymical proceſs can effect. The chyle is compounded of the gaſtric juice, and digeſtible ſubſtances when perfectly converted; and it is probable that the quantity of gaſtric juice may be nearly equal to that part of the food which is really changed into chyle; if ſo, it demonſtrates the neceſſity of a very quick ſecretion, to ſupply a quantity ſo very conſiderable; but with this advantage, that it is not loſt to the conſtitution.

The progreſs of the converſion of food into chyle, may be often ſeen in the ſtomach of animals at different times after feeding. Fiſhes are good ſubjects on which to make obſervations for this purpoſe, as they ſwallow their food whole; and as that food is commonly fiſh, and often too large to be completely admitted into the ſtomach. As they do not maſticate their food, it is not adapted to the cavity of the ſtomach; and therefore part of it is often found lying in the œſophagus; a circumſtance by which the comparitive progreſs of digeſtion is rendered more obvious.

It may alſo be well obſerved in the ſtomach of a dog, in which the whole quantity taken has been ſwallowed at once. In the great end the food will be but little altered; towards the middle, more; and towards the pylorus, it will be ſimilar to what is found in the duodenum.

From the ſtructure of the ſtomach in ruminating animals, they are badly adapted to aſſiſt our inquiries on this ſubject: becauſe metalic balls, or whatever is ſwallowed in ſo hard and ſolid a form, as to be unfit for digeſtion, requiring to be ruminated, will often be thrown out when returned into the mouth for that purpoſe; or it may lie a long time in the firſt ſtomach without being either thrown up or paſſed into the fourth, as I have frequently ſeen; therefore the chance of its getting into the fourth ſtomach in a proper time to fit it for the object of an experiment, being very uncertain, no great light can be derived from trials made on animals of this claſs.

Live or freſh vegetables, when taken into the ſtomach, are firſt killed, by which a flabbineſs in their texture is produced, as if they had been boiled; and then they can be acted upon by the gaſtric juice.

Meat appears to undergo no change, as preparatory to digeſtion; but at once to ſubmit to its union with the gaſtric juice: for after having been acted upon, it ſeems firſt to loſe its texture; then becomes cineritious in colour; next gelatinous; and laſt, chyle. The firſt change made upon milk, and ſome other ſecretions, as the yolk and white of an egg, is coagulation; after which the gaſtric juice begins to acquire a power of uniting with them.

The firſt change which is produced on animal ſubſtances, out of the body, either by being expoſed to heat, or by becoming ſpontaneouſly putrid, is ſimilar to the ſecond of the three changes which takes place in digeſtion; and is only preparatory to the complete change, whether that be digeſtion or putrefaction.

It appears from many experiments, that the digeſted or animalized part, when carried into the inteſtine, is attracted by the villous coat, or clings to it as if entangled among the villi; while the excrementitious part, ſuch as bile, is found lying unconnected in the gut, as if ſeparated from the other.

The food of animals in general conſiſts of vegetable or animal ſubſtances; and vegetables ſeem intended to ſupport one claſs, with a view to its being the food of another. Although there are claſſes of animals intended to ſubſiſt on each particular kind of food, yet they do not all invariably keep to the ſame kind in every ſtage of life; many being nouriſhed by animal food when young, that afterwards live on vegetables: which circumſtance will be more fully diſcuſſed when treating of the firſt food of pigeons.

All ſtomachs do not equally digeſt the ſame ſubſtance, although it be their natural food. The caterpillar digeſts the expreſſed juice, but not the ſubſtance; while other animals are capable of diſſolving nearly the whole. Some animals, as the common cattle, can feed on a variety of vegetables, although they may have a preference; but there are others

that will hardly eat of more than one kind. Of this laſt ſort are inſects in general; and the ſilk-worm will ſcarcely touch any thing but mulberry leaves: but I believe thoſe that live upon animal food are not ſo reſtricted in their choice.

It is probable that, all animal and vegetable ſubſtances are equally capable of being digeſted, if equally ſoft in their texture; but ſome being much firmer in that reſpect, and others alſo united with indigeſtible matter, as the earth in bones, more ſtrongly reſiſt the powers of the gaſtric juice; therefore maſtication, and trituration, become neceſſary to bring them to a ſimilar conſiſtence. But ſubſtances may be rendered too ſoft; for a fluid is difficult of digeſtion; and we may obſerve that, Nature having given us very few fluids as articles of food, to render theſe few fitter for the action of the digeſtive powers, a coagulating principle is provided to give them ſome degree of ſolidity*. It is not eaſy to aſſign a reaſon why fluidity ſhould be unfavourable to the proceſs of digeſtion; more eſpecially as it ſeems eſſential to thoſe of fermentation and chymical ſolution. The requiſite degree of ſolidity, I ſhould ſuppoſe to be, that of curd, or what is produced by the coagulation of animal mucilages, as of the white of an egg; but this is only ſuppoſition, founded on the idea that Nature's general principles are right, and all the correſponding parts adapted to one another, except when monſtrous, either in form or action.

Maſtication is the effect of a mechanical power, produced by parts particularly provided for that purpoſe, which are of various kinds, fitted for that ſort of food on which the animal is by Nature intended to live; and may be imitated with equal advantage by many other pieces of mechaniſm.

The maſticating powers are of three kinds. The firſt is that which merely fits the ſubſtance for deglutition, as in the lion, and many other carnivorous animals; and which, in the ruminating tribe, renders the food fit to be ſwallowed, that it may undergo ſuch preparation in the firſt ſto-

* The circumſtance of the chryſtalline humour, which is ſolid, being coagulated, prior to its being digeſted, renders it probable that all animal ſubſtances go through that proceſs; and that the loſs of texture, which they undergo, ariſes from coagulation.

mach as is neceſſary before it is further maſticated for digeſtion. The ſecond is that, which not only fits the food for deglutition, but expoſes it to the action of the gaſtric juice, by breaking the ſhells or huſks in which the nouriſhment is contained, and in which it would be defended from the powers of digeſtion. And the third is, that which divides and bruiſes the food, before it is received into the ſtomach; which maſtication is of conſiderable ſervice, by producing a ſaving in food.

The huſk, of the ſeeds of plants, although a vegetable ſubſtance, appears to be indigeſtible in its natural ſtate; whether this ariſes from the nature of the huſk itſelf, or from its compactneſs, I am not quite certain, but am inclined to ſuppoſe the laſt; as we find the cocoa, which is only a huſk, to be digeſtible when ground to a powder and well boiled. We know likewiſe, that cuticle, horn, hair, and feathers, although animal ſubſtances, are not affected, in the firſt inſtance, by the gaſtric juice; yet if reduced in Papin's digeſtor to a jelly, that jelly can be acted upon in the ſtomach; we muſt therefore ſuppoſe that, a certain natural degree of ſolidity in animal and vegetable ſubſtances renders them indigeſtible. This compactneſs in the huſk ſeems to be intended to preſerve, while under ground, the farinaceous part of the ſeed, in which the living principle is placed; the huſk having probably no other power of reſiſting putrefaction than what ariſes from its texture; but whatever may be the uſe of the huſk, it muſt be connected with the vegetative proceſs of the plant. The ſame purpoſe of preſervation is probably anſwered by the ſhells of all ova. Although huſks are not capable of being diſſolved in the gaſtric juice, they allow of tranſudation; and that the ſeed is in ſome degree affected by it, is known by its ſwelling in the ſtomach; yet it can only take up a certain proportion of it, and that not ſufficient to convert it into chyle; the gaſtric juice having no power of action upon the huſks themſelves; therefore we ſee grain of all kinds, when ſwallowed whole, paſs through entire, though ſwelled; and even the kernels of ſome nuts, as cheſnuts, are not digeſtible when eat raw.

The eſſential oils of vegetables and animals are indigeſtible; but being ſoluble either in the gaſtric juice or chyle, they become medicinal, from

their ſtimulating powers. The eſſential oil of vegetables, but more particularly that of animals, ſeems to pervade the very ſubſtance of thoſe animals whoſe food contains much of this oil. Thus, we find ſea-birds, whoſe conſtant food is fiſh, taſte very ſtrongly of fiſh; and thoſe who live on that kind of food, only during certain times of the year, as the wild duck, have that taſte only at ſuch ſeaſons. This fact is ſo well known, that it was hardly neceſſary to put it to the teſt of an experiment; yet I took two ducks, and fed one with barley, the other with ſprats, for about a month, and killed both at the ſame time; when they were dreſſed, the one fed wholly with ſprats was hardly eatable, it taſted ſo ſtrongly of fiſh.

Although bones are in part compoſed of animal ſubſtance, and ſo far digeſtible, yet they require ſtronger powers to digeſt them than common meat, from the animal ſubſtance being guarded by the earth. Thus the animal part of a bone is leſs eaſily ſoluble in an alkali than fleſh, or than even the animal part when deprived of its earth by an acid; nor will a bone, being guarded by the calcarious earth, ſubmit to putrefaction ſo readily as meat; therefore animals who live upon others, and ſwallow them whole, as the heron, digeſt bone with more eaſe, than the crow or magpye that are not accuſtomed to ſwallow bones, but commonly pick the fleſh only.

The degree of eaſe, or difficulty, with which ſubſtances are digeſted, will not only ariſe from a difference in ſolidity, but from a difference in the ſtructure of the parts themſelves: brain, liver, muſcle, and tendon, being digeſtible in the order here put down.

There is not only a difference in the degree of facility with which the various kinds of natural food are digeſted; but theſe can alſo be made to undergo changes by art, which render them ſtill more eaſy of digeſtion: for it appears from my experiments, that boiled and roaſted, and even putrid, meat is eaſier of digeſtion than raw, which in the two firſt, may be ſuppoſed to ariſe from their juices being coagulated; but the ſame reaſon will not hold good with regard to the putrid. A raw egg is thought more eaſy of digeſtion than an egg hard boiled, although the raw one muſt be coagulated in the ſtomach before it can be digeſted: and it has like-

wiſe been obſerved, that what is eaſy of digeſtion in one ſtomach will not be ſo in another; but ſuch caſes may probably ariſe from the ſtomach not being in a healthy ſtate.

In many animals the whole of the food does not appear to be digeſted, the ſubſtance in part being found in the fæces; for if a dog is fed with tallow, his excrements will conſiſt of a ſomewhat firm unctuous ſubſtance; ſo that the oil is only digeſted in part. The circumſtance of ſome part of the food, though digeſtible, not being acted upon by the gaſtric juice, may ariſe from two cauſes; firſt, from many parts of vegetables being too firm in texture to be digeſted in the ſame time with the other food, and being therefore carried along in a crude ſtate, together with the chyle, into the duodenum; and ſecondly, from the ſtomach at the time being ſo much diſordered as to digeſt imperfectly. We know that food may lie a conſiderable time in the ſtomach, when it is diſeaſed, without being digeſted. Food has been retained in the ſtomach twenty-four hours, and thrown up without being in the leaſt altered; the animal at the time not requiring nouriſhment: this often ariſes from diſeaſe, and is alſo the caſe with thoſe who go to reſt in the winter.

The powers of digeſtion may, in ſome inſtances, be eſtimated by the appearance of the excrement, in which if the food appears not to be much altered, we may conclude, that digeſtion has had little or no influence on it. Thus the excrement of a flea, that has lived on blood, is nearly to appearance pure blood, not having even loſt its colour.

Animals take food in proportion to the quantity of nouriſhment contained in it, of which the ſtomach appears from inſtinct, to be capable of judging; and alſo in proportion to the powers they poſſeſs of converting what they eat into chyle. A caterpillar, perhaps, eats more in proportion to its ſize than any other animal that lives on the ſame kind of food; for not having the power of diſſolving the vegetable, but only of extracting a juice or infuſion from it; the bit of leaf comes away entire, coiled up and hardened; but by being put into water unfolds like tea.

There are few animals that do not eat fleſh in ſome form or other, while there are many who do not eat vegetables at all; and therefore the

difficulty to make the herbivorous eat meat, is not ſo great, as to make the carnivorous eat vegetables. Where there is an inſtinctive principle in an animal, directing it either to the one ſpecies of food or the other, the animal will certainly die, rather than break through, of its own accord, that natural law; but it may be made to violate every natural principle by artificial means. That the hawk tribe can be made to feed upon bread, I have known theſe thirty years; for to a tame kite I firſt gave fat, which it eat very readily; then tallow and butter; and afterwards ſmall balls of bread rolled in fat or butter; and by decreaſing the fat gradually, it at laſt eat bread alone, and ſeemed to thrive as well as when fed with meat. This, however, produced a difference in the conſiſtence of the excrements; for when it eat meat they were thin, and it had the power of throwing them to ſome diſtance; but when it eat bread, they became firmer in texture, and dropped like the excrement of a common fowl. Spallanzani attempted, in vain, to make an eagle eat bread by itſelf; but by encloſing the bread in meat, ſo as to deceive the eagle, the bread was ſwallowed, and digeſted in the ſtomach.

The excrements of animals we may ſuppoſe to be that part of the common food which is indigeſtible; and as food is either animal or vegetable, and each different kind adapted to diſtinct claſſes of animals, it is natural to believe that the excrementitious part of each will be different; and where the animal feeds upon both, that the excrement will be of a mixed nature. Although this appears probable, it is only true in a certain degree; for the mode of digeſtion, and whether the animal has a cæcum and colon, with their peculiar form, have all an influence in the changes which the food undergoes. Vegetable food produces more excrement than animal, and this according to the kind or parts of vegetables that are eaten. The woody parts and huſks, which are indigeſtible, produce the moſt; the true farinaceous part the leaſt: why there ſhould be any at all from the farinaceous, and animal ſubſtance, except what has eluded the action of the digeſtive organs, is not eaſily accounted for.

All fæces have a tendency to putrefaction, but leaſt in thoſe animals who feed on vegetables. Indeed, the excrement from vegetable food alone

could hardly ever become putrid, if it was not mixed with the mucus of the inteſtines; and would even then be kept ſweet by the tendency which undigeſted vegetables have to take on the vinous and acetous fermentation. But the fæces of thoſe which live entirely on animal food, in general very ſoon become putrid; and indeed often before they are voided: but ſuch animals are either without cæcum or colon; or if not, what they have is very ſhort; ſo that the excrement not being long retained, has leſs time to become putrid. When the fæces ſtagnate ſo as to take on either the vinous or putrefactive fermentation, air is let looſe, which will be according to the nature of the fermentation; moſt probably, from the vegetable it will be fixed; and from the animal, inflammable air.

The fæces of the greateſt number of animals are tinged by the bile, which in ſome gives them a yellowiſh green colour; in the bird they are generally green, but ſometimes white, from being mixed with the urine. The fæces of the maggot appear to be loaded with bile; for beſides being yellow, they are extremely bitter; which is known by eating the kernel of a nut that has a maggot in it. Some kinds of food, when not wholly digeſted, give a tinge to the fæces, as graſs to the excrement of cows.

The animals which feed upon vegetables alone, commonly have their fæces ſomewhat ſolid; but the degree will vary according to the ſtate of the vegetable, whether green or dried; and therefore the particular ſtate of the fæces will depend on the nature of the indigeſtible part of the food, and muſt be different according to the digeſtive powers in different animals. An animal that feeds upon graſs, has the fæces much ſofter than when fed on the ſame kind of graſs made into hay; and therefore the fæces of the herbivorous animals are ſofter in the ſummer than the winter: but green vegetable food does not produce ſoft fæces in all animals; for the caterpillar, which lives upon the leaves of vegetables, has its fæces almoſt dry; and we find in ſome ruminating animals, as ſheep, that the difference in the fæces, during ſummer and winter, is inconſiderable. The quadrupeds, and birds, that live principally upon vegetables, generally have their cæca large, and the colon long, as we ſee in many of the ruminating animals. Some have the colon both long and large, as the horſe,

and thoſe of the rat tribe; which circumſtance has conſiderable effects in allowing the fæces to become dry: in a few of the ruminating animals, and of the rat kind, they are formed into ſmall portions.

The fæces of quadrupeds, living upon animal food, are commonly ſoft, and in birds are fluid; but in ſuch as live on both animals and vegetables, they are in conſiſtence of a mixt nature, and will be more or leſs ſoft, according to the food. If a dog is fed entirely on animal ſubſtance, its fæces will be ſoft; if wholly on vegetable, as on bread, they will become ſo hard as to be expelled with difficulty.

Spallanzani made ſome experiments, to prove that digeſtion is carried on after death; but they are not ſo conducted as to correſpond with the appearances met with in the dead body, where that proceſs has taken place, and the coats of the ſtomach itſelf have been in part digeſted. An experiment, although it may be very well and accurately made, ſo far as the experiment goes, if a cloſe connection is not preſerved with the purpoſe for which it was made, the concluſions to be drawn from it cannot correſpond with the intention. This is exactly the caſe with the experiments of Spallanzani; which, although they prove that meat was digeſted in the ſtomach after the animal was killed, (which no one doubted) yet are not at all calculated to ſhow that the ſtomach itſelf may be digeſted. In fact, the mode in which they were managed, rather tended to prevent that effect from taking place, for the gaſtric juice, by having ſubſtances introduced on which it could act, was leſs likely to affect the coats of the ſtomach. That the digeſtion was not carried on merely by the gaſtric juice ſecreted before the animal was apparently dead, is evident from his own account, ſome of the food which had been introduced and digeſted, being found in the duodenum; a thing that could not have happened, if a ceſſation of the actions of life in the involuntary parts had taken place when viſible life terminated. There had been an action, and moſt probably a ſecretion, in the ſtomach. The only experiment that can be made with any probability of a decided reſult is, to kill the animal while the ſtomach is empty, and obſerve what afterwards takes place. There are few ſtomachs that do not ſhew, when examined after

death, ſome of the inner villous coat deſtroyed; which may have been done by the gaſtric juice in the ducts of the glands which ſecrete it.

Dr. Stevens, in an inaugural diſſertation on this ſubject, publiſhed at Edinburgh 1777, gives a number of experiments, ſome of which are well deviſed, to aſcertain the ſubſtances that are eaſieſt of digeſtion; a thing in fact more wanted, than the cauſe of that proceſs: but many of his experiments, more eſpecially thoſe on ruminating animals, were not made with ſufficient accuracy. How the chopped hay and pot-herbs came to be ſo much changed in the firſt ſtomach of a ruminating animal I cannot conceive, as I have reaſon to believe it has not the leaſt power of digeſting; and ſhould doubt very much that hay was capable of being wholly digeſted in any ſtomach. His experiment made on ſubſtances out of the body, proves that the gaſtric juice is not able in all caſes to prevent the vinous and acetous fermentation in vegetables; and is a circumſtance which I believe often takes place in the living body, when the ſtomach is weak. He ſeems to be in ſome apprehenſion for the ſafety of the ſtomach itſelf, from the action of ſo powerful a ſolvent as the gaſtric juice: but though inclined to ſuppoſe that the living powers of the animal may guard it againſt ſuch effects; yet he is ſtill diſpoſed to fear that, in all caſes they may not be ſufficient.

The living power, in the ſtomach, muſt indeed be very weak to admit of its being digeſted; where that was likely to happen, I imagine the ſecretion of the gaſtric juice would be too defective to allow of the ſtomach being acted upon.

Dr. Stevens gives two caſes, with the diſſections, to prove that the living ſtomach has not always the power to reſiſt the action of the gaſtric juice: but he has not made it clear, that thoſe very ſtomachs might not have been digeſted after death. The appearance of the edges of the hole ſhould have been more particularly deſcribed; for if it took place before death, it is probable it was owing to ulceration, which I have ſometimes ſeen. Men ſhould be very accurate in aſcertaining the truth of facts, before they advance them, eſpecially when they tend either to overturn a received opinion, or to eſtabliſh a new one. As to the poſſibility of ani-

mals ſwallowed alive being digeſted, no freſh proofs are neceſſary, as we eat oyſters every day; but this does not prove that they are digeſted while alive. In his experiments made on ruminating animals, and the dog, as the vegetables were not ſo readily digeſted as the meat, he concludes, " It is poſſible every ſpecies of animal has its peculiar gaſtric liquor, capable of diſſolving certain ſubſtances only"; which is certainly not true.

Mr. Senebier relates ſome experiments made by Mr. Goſſe, upon himſelf; but which hardly contain any thing, except a curious conjecture of Mr. Senebier's, " That diſtention of the ſtomach is the cauſe of the ſecretion of the gaſtric liquor." He mentions the ſubſtances, both animal and vegetable, which are not digeſtible; then thoſe difficult of digeſtion; afterwards, thoſe eaſily digeſted; alſo what ſubſtances facilitate digeſtion, and what retard it. But if we are to judge of the truth of theſe facts from a detail of the experiments which he made to aſcertain them, I am inclined to believe that the experiments have not been made with ſufficient accuracy to be depended upon.

ON THE DIGESTION OF THE STOMACH AFTER DEATH.

THE following account, of the Stomach being digeſted after Death, was drawn up at the deſire of the late Sir John Pringle, when he was preſident of the Royal Society; and the circumſtance which led to it was as follows: I had opened, in his preſence, the body of a patient who had been under his care, in which the ſtomach was found to be in part diſſolved; a thing that appeared to him very unaccountable, there having been no previous ſymptom which could have led him to ſuſpect any diſeaſe in the ſtomach. I took that opportunity of explaining to him my ideas reſpecting it; and that having long been employed in making experiments on digeſtion, I had been induced to conſider this as one of the facts which proved a converting power in the gaſtric juice. I mentioned my intention of publiſhing the whole of my obſervations on digeſtion at ſome future period; but he deſired me, in the mean time, to give this

fact by itſelf, with my remarks; as it would prove that there is a ſolvent power exiſting in the ſtomach, and would be of uſe in the examination of dead bodies.

An accurate knowledge of the appearances in animal bodies, where death has been the conſequence of ſome violence while they were otherwiſe in health, ought certainly to be conſidered as neceſſary to qualify us to judge truly of the ſtate of the body in thoſe that die of diſeaſes. An animal body undergoes changes after death; but it has never been ſufficiently conſidered what thoſe changes are, or how ſoon they may take place; yet till this be done, it is impoſſible we can form an accurate judgment of the appearances which preſent themſelves at the time of inſpection. The diſeaſes of an animal body (mortification excepted) are always connected with the living principle, and are not in the leaſt ſimilar to the changes which take place in the dead body: without a knowledge of this, an opinion drawn from diſſections muſt always be very imperfect, or very erroneous. Appearances which are in themſelves natural, may be miſtaken for thoſe of diſeaſe: we may ſee diſeaſed parts, and ſuppoſe them in a natural ſtate; we may conſider a circumſtance to have exiſted before death, which was really a conſequence of it; or we may imagine it to be a natural change after death, when it was in fact a diſeaſe of the living body. It is eaſy to ſee, therefore, how a man in this ſtate of ignorance muſt blunder, when he comes to connect the appearances in a dead body with the ſymptoms that were obſerved in life; and indeed, all the advantage to be derived from opening dead bodies, depends upon the judgment and ſagacity with which this ſort of compariſon is made.

There is a caſe of a mixed nature, which can neither be reckoned a proceſs of the living body, nor of the dead; it participates of both, inaſmuch as its cauſe ariſes from life, and the effect cannot take place till after death. To render this more intelligible, it will be neceſſary to ſtate ſome general ideas concerning this cauſe and effect.

An animal ſubſtance, when joined with the living principle, cannot undergo any change in its properties but as an animal; this principle al-

ways acting and preserving the subſtance poſſeſſed of it from diſſolution, and from being changed, according to the natural changes which other ſubſtances undergo.

There are a great many powers in nature, which the living principle does not enable the animal matter, with which it is combined, to reſiſt, viz. the mechanical and moſt of the ſtrongeſt chymical ſolvents. It renders it, however, capable of reſiſting the powers of fermentation, digeſtion, (and perhaps ſeveral others) which are well known to act on this ſame matter, and entirely to decompoſe it, when deprived of the living principle. The number of powers which thus act differently on the living and dead animal ſubſtance not being aſcertained, we ſhall only take notice of two, putrefaction and digeſtion, which do not affect this ſubſtance, unleſs when it is deprived of the living principle. Putrefaction is an effect which ariſes ſpontaneouſly; digeſtion is an effect of another principle, and ſhall here be conſidered a little more particularly.

Animals, or parts of animals, poſſeſſed of the living principle, when taken into the ſtomach, are not in the leaſt affected by the powers of that viſcus, ſo long as the animal principle remains; hence it is that we find animals of various kinds not only can live in the ſtomach, but are even hatched and bred there: yet the moment that any of theſe loſe the living principle, they become ſubject to the digeſtive powers of the ſtomach. If it were poſſible for a man's hand, for example, to be introduced into the ſtomach of a living animal, and kept there for ſome conſiderable time, it would be found, that the diſſolvent powers of the ſtomach could have no effect upon it; but if the ſame hand were ſeparated from the body, and introduced into the ſame ſtomach, we ſhould then find that the ſtomach could immediately act upon it. Indeed, if the firſt were not the caſe, the ſtomach itſelf ought to have been made of indigeſtible materials; for were not the living principle capable of preſerving animal ſubſtances from being acted upon by the proceſs of digeſtion, the ſtomach itſelf would be digeſted: and accordingly we find that the ſtomach, which at one inſtant, that is, while poſſeſſed of the living principle, was capable of reſiſting the digeſtive powers which it contained, the next moment, viz.

when deprived of the living principle, is itſelf capable of being digeſted, not only by the digeſtive powers of other ſtomachs, but even by the remains of that power which itſelf had of digeſting other things.

Theſe obſervations lead us to account for an appearance which we often find in the ſtomachs of dead bodies; and they at the ſame time throw conſiderable light upon the nature of digeſtion. The appearance we allude to, is a diſſolution of the ſtomach at its great extremity; in conſequence of which, there is frequently a conſiderable apperture made in that viſcus. The edges of this opening appear to be half diſſolved, very much like that kind of ſolution which fleſhy parts undergo when half digeſted in a living ſtomach, or when acted upon by a cauſtic alkali, viz. pulpy, tender, and ragged.

In theſe caſes the contents of the ſtomach are generally found looſe in the cavity of the abdomen, about the ſpleen and diaphragm; and in many ſubjects the influence of this digeſtive power extends much further than through the ſtomach. I have often found, that after the ſtomach had been diſſolved at the uſual place, its contents let looſe had come into contact with the ſpleen and diaphragm, had diſſolved the diaphragm quite through, and had partly affected the adjacent ſide of the ſpleen, ſo that what had been contained in the ſtomach, was found in the cavity of the thorax, and had even affected the lungs to a ſmall degree.

There are very few dead bodies in which the ſtomach at its great end is not in ſome degree digeſted; and one who is acquainted with diſſections can eaſily trace theſe gradations. To be ſenſible of this effect, nothing more is neceſſary than to compare the inner ſurface of the great end of the ſtomach with any other part of its inner ſurface; the ſound portions will appear ſoft, ſpongy, and granulated, and without diſtinct bloodveſſels, opake and thick; while the others will appear ſmooth, thin, and more tranſparent; and the veſſels will be ſeen ramifying in its ſubſtance, and upon ſqueezing the blood which they contain from the larger branches to the ſmaller, it will be found to paſs out at the digeſted ends of the veſſels, and to appear like drops on the inner ſurface.

Though I have often ſeen ſuch appearances, and ſuppoſed that they

muſt have been ſeen by others, yet I was quite at a loſs to account for them. At firſt, I ſuppoſed them to have been produced during life, and was therefore inclined to look upon them as the cauſe of death; only that I never found they had any connection with the patient's ſymptoms: but I was ſtill more at a loſs to account for them, when I diſcovered they were moſt frequent in thoſe who died by ſudden violence; a circumſtance which made me ſuſpect that the true cauſe was not gueſſed at*.

At this time I was employed in making experiments upon digeſtion in different animals; all of which were killed at different times, after having been fed with various kinds of food; many of theſe were not opened immediately after death, and in ſome of them I found the above deſcribed appearances in the ſtomach. The better to purſue my inquiry on the ſubject of digeſtion, I procured the ſtomachs of a vaſt variety of fiſhes, whoſe deaths are always violent; and who may be ſaid to die in perfect health, with their ſtomachs uſually full. In them we can obſerve the progreſs of digeſtion moſt diſtinctly; the ſhape of their ſtomachs being very favourable for that purpoſe. They likewiſe ſwallow their food whole, that is, without maſtication; and ſwallow fiſh that are much larger than the digeſting part of the ſtomach can contain; therefore in many inſtances the part ſwallowed, which was lodged in the digeſting part of the ſtomach, was found more or leſs diſſolved, while that which remained

* The firſt time that I had occaſion to obſerve this appearance, where death had been produced by violence, and where it could not therefore eaſily be ſuppoſed to be the effect of diſeaſe, was in a man who had his ſkull fractured by one blow of a poker. Juſt before this accident he had been in perfect health, and had taken a hearty ſupper of cold meat, cheeſe, bread, and ale. Upon opening the abdomen, I found that the ſtomach, though it ſtill contained a good deal, was diſſolved at its great end, and a conſiderable part of its contents lay looſe in the general cavity of the belly; a circumſtance which puzzled me very much. The ſecond inſtance was in a man who died at St. George's Hoſpital, a few hours after receiving a blow on his head, which fractured his ſkull. From theſe two caſes, among various conjectures about ſo ſtrange an appearance, I began to ſuſpect it might be peculiar to caſes of fractured ſkull; and therefore, whenever I had an opportunity, I examined the ſtomach of every perſon who died from that accident; but I found many of them which had not this appearance. I afterwards met with the ſame appearance in a man who had been hanged.

in the œſophagus was perfectly ſound: and in many of theſe I ſaw the digeſting part of the ſtomach itſelf reduced to the ſame diſſolved ſtate as the digeſted part of the food.

Being employed upon this ſubject, and therefore enabled to account more readily for appearances which had any connection with it, and obſerving that the half-diſſolved parts of the ſtomach were ſimilar to the half-digeſted food, it immediately ſtruck me, that it was the proceſs of digeſtion going on after death; and that the ſtomach, being dead, was no longer capable of reſiſting the powers of that menſtruum, which itſelf had formed for the digeſtion of food.

Theſe appearances of the ſtomach after death, throw conſiderable light on the principles of digeſtion, and ſhow, that it neither depends on a mechanical power, nor contractions of the ſtomach, nor on heat, but ſomething ſecreted in the coats of the ſtomach, and thrown into its cavity, which there animaliſes the food, or aſſimilates it to the nature of the blood. The power of the gaſtric juice is confined or limited to certain ſubſtances, generally of the vegetable and animal kingdoms; and although this menſtruum is capable of acting independently of the ſtomach, yet it is indebted to that viſcus for its exiſtence and continuance.

Plate XIV

Wm. Bell del.

W. Skelton sculp.

PLATE XIV.

A portion of inteſtine of a hog, the peritoneal coat of which is covered in ſeveral places with ſmall pellucid cyſts, containing air.

It was ſent to me by my friend Mr. Jenner, ſurgeon at Berkley, who informed me, that this appearance is found very frequently upon the inteſtines of hogs that are killed in the ſummer months.

A The portion of the meſentery.
B The portion of inteſtine on which the air-cells are ſituated.

ON A SECRETION IN THE CROP OF BREEDING PIGEONS, FOR THE NOURISHMENT OF THEIR YOUNG.

THE nouriſhment of animals admits, perhaps, of as much variety in the mode by which it is to be performed, as any circumſtance connected with their œconomy; whether we conſider their numerous tribes, the different ſtages through which every animal paſſes, or the food adapted to the ſupport of each, in their diſtinct conditions and ſituations. We are likewiſe to include in this view, that endleſs variety, in the means by which this food is procured, according to the claſs of the animal and the particular ſtage of its exiſtence. If the food was the ſame through every period of the life of an animal; if every individual of a tribe lived on the ſame kind, and procured it by the ſame mode, our ſpeculations would then admit of a regular arrangement. But when we ſee that the food adapted to one ſtage of an animal's life is rejected at another; and that animals of one claſs in ſome reſpects reſemble thoſe of another, by hardly having any food peculiar to themſelves, the ſubject becomes ſo complicated, that it is not ſurpriſing if we are at a loſs to arrange the various modes by which animals are nouriſhed.

Animal life may not improperly be divided into three ſtates, or ſtages. The firſt comprehends the production of the animal and its growth in the fœtal ſtate: the ſecond commences when it emerges from that ſtate by what is called the birth; yet for a certain time muſt, either mediately or immediately, depend on the parent for ſupport; the third may be ſaid to take place when the animal is fit, and at liberty, to act for itſelf. The firſt and third ſtages are perhaps common to all animals; but there are ſome claſſes, as fiſhes, ſpiders, &c. which ſeem to have no ſecond ſtage, but paſs directly from the firſt to what is the third in other animals. Of

thoſe requiring a ſecond ſtage, the polypus and the viviparous animals continue to derive their nouriſhment immediately from the parent; while the oviparous are for ſome time ſupported by a ſubſtance originally formed with them, and reſerved for that purpoſe.

There is infinite variety in the means by which Nature provides for the ſupport of the young in the ſecond ſtage of animal life. In many inſects it is effected by the female inſtinctively depoſiting the egg, or whatever contains the rudiments of the animal, in ſuch a ſituation that, when hatched, it may be within reach of proper food: others, as the humble-bee and blackbeetle, collect a quantity of peculiar ſubſtance, which both ſerves as a nidus for the egg, and nouriſhment for the maggot, when the embryo arrives at that ſtate. Moſt birds, and many of the bee tribe, collect food for their young; when at a more advanced period, the taſk of feeding them is performed by both male and female, with an exception in the common bee, the young ones of which are not fed by either parent, but by the working bees, who act the part of the nurſe. There is likewiſe a number of animals capable of ſupplying immediately from their own bodies, the nouriſhment proper for their offspring, during this ſecond ſtage; a mode of nouriſhment which has hitherto been ſuppoſed to be peculiar to that claſs of animals which Linnæus calls Mammalia; nor has it, I imagine, been ever ſuſpected to belong to any other.

I have, however, in my inquiries concerning the various modes in which young animals are nouriſhed, diſcovered that all of the dove kind are endowed with a ſimilar power. The young pigeon, like the young quadruped, till it is capable of digeſting the common food of its kind, is fed with a ſubſtance ſecreted for that purpoſe by the parent animal: not as in the Mammalia by the female alone, but alſo by the male; which, perhaps, furniſhes this nutriment in a degree ſtill more abundant. It is a common property of birds, that both male and female are equally employed in hatching, and in feeding their young in the ſecond ſtage; but this particular mode of nouriſhment, by means of a ſubſtance ſecreted in their own bodies, is peculiar to certain kinds, and is carried on in the crop.

Beſides the dove kind, I have ſome reaſon to ſuppoſe parrots to be

endowed with the ſame faculty, as they have the power of throwing up the contents of the crop, and feeding one another. I have ſeen the cock paroquet regularly feed the hen, by firſt filling his own crop, and then ſupplying her from his beak. Parrots, macaws, cockatoos, &c. when they are very fond of the perſon who feeds them, may likewiſe be obſerved to have the action of throwing up the food, and often do it. The cock pigeon, when he careſſes the hen, performs the ſame kind of action as when he feeds his young; but I do not know, if at this time, he throws up any thing from the crop.

During incubation, the coats of the crop, in the pigeon, are gradually enlarged and thickened, like what happens to the udder of females of the claſs Mammalia, in the term of uterine-geſtation. On comparing the ſtate of the crop when the bird is not ſitting, with its appearance during incubation, the difference is very remarkable. In the firſt caſe it is thin and membranous; but by the time the young are about to be hatched, the whole, except what lies on the trachea, becomes thicker and takes on a glandular appearance, having its internal ſurface very irregular*. It is likewiſe evidently more vaſcular than in its former ſtate; that it may convey a quantity of blood, ſufficient for the ſecretion of the ſubſtance, which is to nouriſh the young brood for ſome days after they are hatched.

Whatever may be the conſiſtence of this ſubſtance, when juſt ſecreted, it moſt probably very ſoon coagulates into a granulated white curd; for in ſuch a form I have always found it in the crop: and if an old pigeon is killed juſt as the young ones are hatching, the crop will be found as above deſcribed, and in its cavity pieces of white curd mixed with ſome of the common food of the pigeon, ſuch as barley, beans, &c. If we allow either of the parents to feed the brood, the crop of the young pigeons, when examined, will be diſcovered to contain the ſame kind of curdled ſubſtance as that of the old ones; which paſſes from thence into the ſtomach, where it is to be digeſted.

The young pigeon is fed for a little time with this ſubſtance only; as

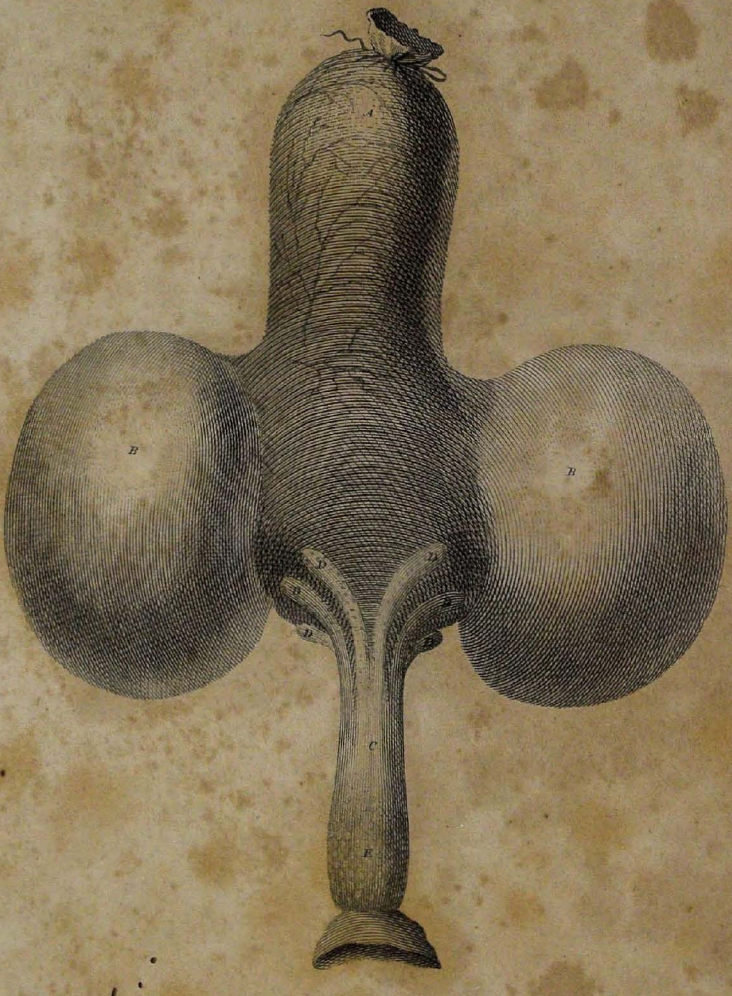
* Vide Plate XVI.

about the third day, ſome of the common food is found mingled with it; as the pigeon grows older, the proportion of common food is increaſed; ſo that by the time it is ſeven, eight, or nine days old, the ſecretion of the curd ceaſes in the old ones, and of courſe no more will be found in the crop of the young. It is a curious fact, that the parent pigeon has at firſt a power to throw up this curd without any mixture of common food, although afterwards both are thrown up according to the proportion required for the young ones.

I have called this ſubſtance curd, not as being literally ſo, but as reſembling that more than any thing I know; it may, however, have a greater reſemblance to curd, than we are perhaps aware of; for neither this ſecretion, nor curd, from which the whey has been preſſed, ſeem to contain any ſugar, and do not run into the acetous fermentation. The property of coagulating is confined to the ſubſtance itſelf, as it produces no ſuch effect when mixed with milk.

This ſecretion in the pigeon, like all other animal ſubſtances, becomes putrid by ſtanding; though not ſo readily as either blood or meat, it reſiſting putrefaction for a conſiderable time; neither will curd, much preſſed, become putrid ſo ſoon as either blood or meat.

Plate XV



Wm. Bell del.

Wm. Skelton sculp.

PLATE XV.

THE crop, taken from a pigeon when it had no young ones.

The crop of the pigeon appears to be placed more in the middle of the neck than in any other bird, being two equal bags, annexed, laterally, to the œſophagus; whereas in moſt other birds, it is ſingle, and a little on one ſide. The œſophagus of thoſe birds which have crops, may be divided into two portions, a ſuperior and inferior. The ſuperior is that which leads from the mouth to the crop; the inferior, from the crop to the gizzard.

The crop, repreſented in the plate, had been turned inſide out, and diſtended with ſpirits. It ſhows the appearance of the internal ſurface.

A The inner ſurface of the ſuperior œſophagus.

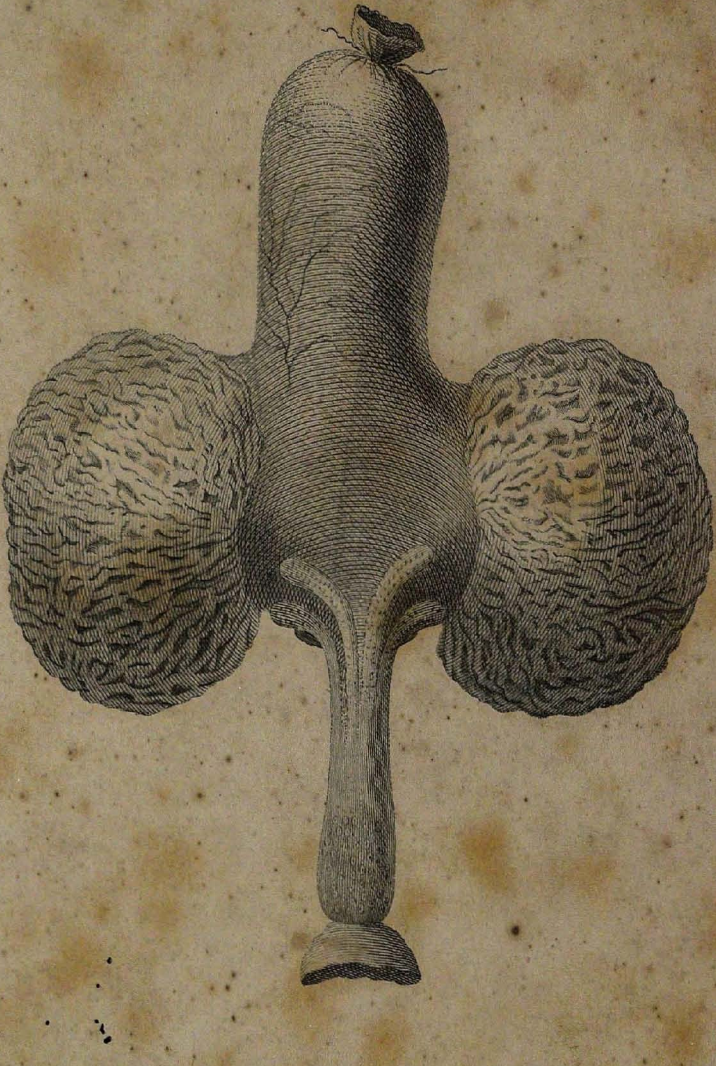
BB The inſide of the two projecting bags of the crop.

E The inferior œſophagus, leading from the crop to the gizzard.

DDDD Glands ſituated on the lower part of the crop, and continued into the inferior œſophagus.

E A glandular ſtructure upon the inner ſurface of this œſophagus, juſt before it terminates in the gizzard, for the purpoſe of ſecreting a ſubſtance analogous to the gaſtric liquor.

Plate XVI



Wm. Bell del.

Wm. Skelton sculp.

PLATE XVI.

THE crop, from a male pigeon, while the female was breeding, to ſhew the change which takes place at that time, on its internal ſurface, for the purpoſe of ſecreting a ſubſtance which is to nouriſh the young.

The crop is prepared in the ſame way as in Plate XV; and the only difference in the appearance is the glandular ſtructure on the inner ſurface of the two lateral projecting bags, which is not ſeen at any other time.

ON THE COLOUR OF THE PIGMENTUM OF THE EYE IN DIFFERENT ANIMALS.

IN the eyes of all animals which I have examined, there is a ſubſtance approaching to the nature and appearance of a membrane, called the pigmentum, which lines the choroid coat, and is ſomewhat ſimilar to the rete mucoſum which lies under the cuticle of the human body; and there is alſo ſome of the ſame kind of ſubſtance diffuſed through the cellular-membrane, which unites the choroid with the ſclerotic coat. My intention, at preſent, is only to communicate the obſervations I have made on this ſubject, and its uſe, confining myſelf to the conſideration of that kind of it which lines the tunica choroides of the claſs Mammalia, and of birds: in doing which I ſhall alſo take occaſion to ſpeak of the difference of colour occurring in animals of the ſame ſpecies. Although an accurate examination of the appearances of a ſimilar ſubſtance in the eyes of ſome fiſhes might illuſtrate the ſubject, we cannot avail ourſelves of that, as from not being ſufficiently acquainted with the effects of light on the eyes of that claſs of animals.

The propagation, or continuance of animals in their diſtinct claſſes, is an eſtabliſhed law of Nature; and, in a general way, is preſerved with a tolerable degree of uniformity: but in the individuals of each ſpecies, varieties are every day produced in colour, ſhape, ſize, and diſpoſition. Some of theſe changes are permanent with reſpect to the propagation of the animal; becoming ſo far a part of its nature as to be continued in the offspring.

Animals living in a free and natural ſtate are ſubject to few deviations from their ſpecific character; but Nature is leſs uniform in its operations

when influenced by culture*. Conſiderable varieties are produced under ſuch circumſtances; of which the moſt frequent are changes in the colour. Theſe changes are always, I believe, from the dark to the lighter tints; and the alteration very gradual in certain ſpecies, requiring in the Canary-bird ſeveral generations; while in the crow, mouſe, &c. it is completed in one. But this change is not always to white, though ſtill approaching nearer to it in the young, than in the parent; being ſometimes to dun, at others to ſpotted, of all the various ſhades between the two extremes. This alteration in colour being conſtantly from dark to lighter, may we not reaſonably infer, that in all animals ſubject to ſuch variation, the darkeſt of the ſpecies ſhould be reckoned neareſt to the original; and that where there are ſpecimens of a particular kind, entirely black, the whole have been originally black? Without this ſuppoſition, it will be impoſſible, on the principle I have ſtated, to account for individuals of any claſs being black. Every ſuch variety may be conſidered as ariſing in the cultivated ſtate of animals: but whether, if left to themſelves, they would in time reſume their original appearance, I do not know†.

The colour of the pigmentum of the eye always correſponds, I believe, with that of the hair and ſkin, eſpecially if the animal be only of one colour, but is principally determined by the hair; and the moſt general colour is a very dark brown, approaching to black, from whence it had the name, nigrum pigmentum‡. The colour differs in different claſſes of animals, often in the ſame claſs, and even in the ſame ſpecies. In the human, it is moſt commonly dark; in the ferret kind, always light: and

* From the variations produced by culture, it would appear, that the animal is ſo ſuſceptible of impreſſion, as to vary Nature's actions; and this is even carried into propagation. Whether this takes place at the very firſt union of the principles of the two parents, ſo as to derive its exiſtence from both; or, whether it takes its formation from the mother, after the firſt formation of the embryo, are, perhaps, not eaſily determined.

† In vegetables, I believe, it invariably holds good, that however improved by culture, if neglected, they ſoon degenerate into their firſt ſtate.

‡ As the colour of this membrane correſponds with the colour of the ſkin and hair of the perſon, it is probable that the people, among whom it firſt got the name, were dark.

its difference of colour in the ſame ſpecies is evident from the variety obſervable in the eyes of different people. There is even a difference of colour in the ſame eye in many claſſes of animals: in all of the cat and dog kind; and perhaps in moſt part of the granivorous. In ſome it is partly black, and partly of the appearance of poliſhed ſilver: and in many claſſes, the variation from dark is of two colours; for in the cow, in ſheep, deer, horſes, and I believe in all animals feeding on graſs, there are, in the ſame eye, certain portions of it white, and others of a fine green colour. The difference in colour of this pigmentum, in the eyes of different animals of the ſame ſpecies, is very remarkable; in the human ſpecies it is of all the different ſhades between black and almoſt white; and the ſame variety is ſeen in rabbits, mice, crows, blackbirds, &c. but in theſe it is of one colour only in the ſame eye. Every ſpecies is, perhaps, ſubject to ſuch variations; and ſome of theſe are ſo extraordinary, as with propriety to be denominated monſtrous*.

The variation in the colour of the pigmentum in different ſpecies of animals, ſeems to depend on a fixed law of Nature; but the varieties which are met with in the ſame ſpecies are much leſs conſtant, being merely different ſhades, approaching to black or white. But the extraordinary circumſtance is, its being ſometimes unuſually lighter or darker in individuals of the ſame ſpecies; and this difference not ſeldom ſtarting up in the young without any hereditary principle to account for it.

The human ſpecies is a ſtriking example of the colour of the pigmentum correſponding with that of the ſkin and hair; and though the ſkin and hair of one perſon differs very conſiderably from the ſkin and hair of another, yet it is not in ſo great a degree as in many animals. There are cattle perfectly white, white ſheep, white dogs, white cats and rabbits;

* Perhaps the word, monſtrous, is too ſtrong, or not exactly juſt. It certainly may be laid down as one of the principles or laws of Nature, to deviate under certain circumſtances. It may alſo be obſerved, that it is neither neceſſary, nor does it follow, that all deviations from the original muſt be a falling off; it appears juſt the contrary, therefore we may ſuppoſe that Nature is improving its works; or, at leaſt, has eſtabliſhed the principle of improvement in the body as well as in the mind.

but there are few of the human ſpecies that we can ſay are perfectly white. They rather paſs from the black into the brown, red, and even light yellow; and we find this pigmentum, although only of one colour, varying through all the different correſponding ſhades. In the African negro, the blackneſs of whoſe hair and ſkin are great diſtinguiſhing characteriſtics, this pigmentum is alſo very black. In the mulatto, who has not the ſkin ſo dark as the African, but the hair nearly as black, this pigmentum is of a ſhade not quite ſo deep, yet ſtill it does not approach ſo near to the middle tint as the ſkin, rather following the colour of the hair. In people of a ſwarthy complexion, as Indians, Turks, Tartars, Moors, &c. we find the hair always of a jet black; and this ſubſtance of a much darker brown than in thoſe that are fair. In thoſe of very dark complexions, and having very black hair, although deſcended from fair parents, the ſame thing holds good. There are few ſpecies of animals, or even individuals of a ſpecie, whoſe bodies are only of one colour. Crows, and ſome others, are exceptions; but the greateſt number are of two or more, being variouſly ſpotted or ſtreaked, either with different colours, or with ſhades of the ſame. Many ſpecies are conſtantly lighter in ſome parts of the body than in others; and, with a few exceptions, animals are generally lighter, as to colour, on the lower, or what may be called the foreparts, than on the upper or backparts. The fair man or woman may ſtrictly be conſidered as a ſpotted or variegated animal. In many perſons, the hair of the head, eyebrows, eyelaſhes, beard, and hair on the pubes, all vary in colour. The hair of the three firſt may be called fœtal, and are oftener all of the ſame, than of a different colour; the two laſt are to be conſidered as adult hair, and are commonly alike in colour, which yet frequently varies from that of the fœtus; which laſt is more liable to change its colour than the other; and the change is generally that of growing darker, eſpecially on the head and the eyelaſhes*. This difference in the colour of the hair, on different parts of the body, is not ſo obſerva-

* The hair growing grey, is not in the leaſt to the preſent purpoſe.

ble in thoſe nations who are dark or ſwarthy, as in people inhabiting many of the northern climates.

In animals which are variegated, let us obſerve the colour of this pigmentum, and we ſhall find it regulated by ſome general principle, and correſponding with the colour of the eyelaſhes. The magpie, for inſtance, is nearly one-third, or fourth part, white; and the two colours, if blended, would make the compound grey; but the eyelaſhes being black, the pigmentum is black alſo. We ſometimes meet with people whoſe ſkin and hair are very white, and yet the iris is dark, which is a ſign of a dark pigmentum; but if we examine more carefully, we ſhall alſo find that the eyelaſhes are dark, although the eyebrows may be the colour of the common hair.

As the colour of the iris in the human ſpecies is probably a preſumptive, though not a certain ſign of the colour of this pigmentum, we may be led to ſuppoſe, that in thoſe who have the iris in one eye different from that of the other, this ſubſtance will likewiſe differ: but this I cannot determine, never having examined the eyes of any perſon with ſuch a peculiarity. It is not an uncommon circumſtance in ſome ſpecies of animals; the Angola cat ſeldom having the colour of the iris the ſame in both eyes.

In people remarkably fair, whether they are of a race that is naturally ſo, or what may be called monſtrous in reſpect to colour, as white Æthiopians, ſtill we find this pigmentum following the colour of the ſkin and hair; being in ſome of a light brown, and in others almoſt white, according to the colour of the hair in ſuch people.

All foals are of the ſame colour; and whatever that may be, as they grow older it generally becomes lighter; therefore the pigmentum in them is almoſt always of the ſame colour, and does not ſeem to change with the hair. This change, however, is only in the hair, and not in the ſkin; the ſkin of a white or grey horſe being as dark as the ſkin of a black one: yet there is a cream-coloured breed which has the ſkin of the ſame colour, whoſe foals are alſo of a cream-colour; and by inſpecting the parts not covered with hair, ſuch as the mouth, anus, ſheath, &c. theſe, and the

pigmentum of the eyes of ſuch horſes, are found of a cream-colour likeiwſe.

In the pigmentum of the rabbit kind, there are all the degrees of dark and light, correſponding with the colour of the hair; yet there ſeem to be exceptions to this rule in ſome white rabbits with black eyes, and therefore with black pigmentum; but in all ſuch there is either a circle of black hair ſurrounding the eye, or the eyelaſhes, and the ſkin forming the edge of the lid, is alſo black. In many white cattle, this is alſo obſervable; and in that breed of dogs, called Danes, ſome have the hair ſurrounding one eye black, while the hair ſurrounding the other is white; and the iris of the one is often lighter than that of the other. This circumſtance, of the iris of one eye being lighter in colour than that of the other, is a common thing in the human ſpecies; and ſometimes only one-half of the iris is light, without any difference in the colour of the eye-laſh, or eyebrow. Whether this difference in the colour of the iris of the two eyes, in the ſame animal, is owing to the pigmentum being different in colour, I do not know; although I rather ſuſpect it is ſomething ſimilar to the white iris in horſes, which makes them what is called wall-eyed.

The variation of colour appears moſt remarkable when a white ſtarts up, either where the whole ſpecies is black, as in the crow or blackbird; or where only a certain part of the ſpecies is black, (but permanently ſo) as a white child born of black parents; and a perfectly white child whoſe hair is white, and who has the pigmentum alſo white, though born of parents who are fair, ſhould as much be conſidered as a play of Nature as the others. All theſe luſus naturæ; ſuch as the white negro, the pure white child of fair parents, the white crow, the white blackbird, white mice, &c. have likewiſe a white pigmentum correſponding with the colour of the hair, feathers, and ſkin.

Beſides the circumſtance of animals of the ſame ſpecies differing from one another in colour, there are ſome diſtinct ſpecies which are, as far as we know, always of a light colour, and in them too this pigmentum is white: the animal I allude too is the ferret.

When the pigmentum is of more than one colour in the ſame eye, the lighter portion is always placed at the bottom of the eye, in the ſhape

of a halfmoon with the circular arch upwards; the ſtraight line or diameter paſſing almoſt horizontally acroſs the lower edge of the optic nerve, ſo that the end of the nerve is within this lighter coloured part, which makes a kind of ſemicircular ſweep above it. This ſhape is peculiar to the cat, lion, dog, and moſt of the carnivorous tribe; in the herbivorous, the upper edge being irregular; in the ſeal, however, the light part of this pigmentum is equally diſpoſed all round the optic nerve, and is, on the whole, broader than it is commonly found in quadrupeds. How far this increaſe of ſurface is an approach towards the fiſh kind, in which it is wholly of this metallic white, I will not pretend to ſay; but it is probable, as the animal is to ſee in the water as well as in the air, that it may be formed circular, the better to correſpond with the form of the eyelids, which open equally all round; which ſeems to accord with what is obſervable in fiſhes, they being without eyelids.

The colour of the pigmentum, whether white, or green, or both, has always a bright ſurface, appearing like poliſhed metal; which appearance animal ſubſtance is very capable of taking on, as we ſee in hair, feathers, ſilk, &c.

After having taken notice of the various colours of this pigmentum in different animals, both where permanent, and where it appears to be a play of Nature, let us next examine what effect it has upon viſion in both caſes; whether theſe effects are ſimilar, or if one caſe illuſtrates the other.

It may be aſſerted as an undoubted fact, that the light which falls on the retina, covering a white pigmentum, has more effect than when it falls on the retina which covers a dark one: which is known by comparing the viſion of thoſe of the ſame ſpecies who have the pigmentum wholly dark, with thoſe who have it perfectly white; and ſomething may be learned, by a ſimilar compariſon of animals who have it only in different ſpecies; it being reaſonable, from analogy, to ſuppoſe that ſome ſuch effect is produced in the eye which is poſſeſſed of both.

I ſhall firſt conſider the effect produced when the white or light colour makes only part of the pigmentum. This will lead me to obſerve, that

all animals having the pigmentum diverſified, though they are capable of bearing as much light as others, and can ſee as perfectly when light is in an equal degree; can likewiſe ſee very diſtinctly when the light is much leſs than will ſerve the purpoſes of animals having it wholly dark. May we not, therefore, aſcribe this advantage to the pigmentum being partly white? One might be almoſt tempted to ſuppoſe, that ſuch animals have a power of preſenting the different parts of the eye to the light, according to the quantity of light required; or of moving the chryſtalline humour higher or lower: but we are at preſent unacquainted with any power in the eye by which theſe actions can be performed.

We may obſerve that when a cat or dog looks at us in the twilight, the whole pupil is inlarged and illuminated; but in a full light there is no ſuch appearance. It is plain there muſt be a reflexion of light from the bottom of the eye, to produce the above effect; eſpecially as the light reflected is always of the colour of the pigmentum in ſuch animals; which in the cow is of a light green.

I ſhall ſecondly, conſider thoſe animals which have the whole pigmentum of a white colour, whether it is accidental or natural; and that ſee much better in the dark, or with leſs light than thoſe in which it is of a dark colour: of the firſt of theſe I ſhall take my inſtance from the human ſpecies; of the ſecond, the ferret will ſerve as an example.

Thoſe of the human ſpecies, who have the pigmentum of a light colour, ſee much better with a leſs degree of light than thoſe who have it dark; and this in proportion to their fairneſs: for when the hair is quite white, they cannot ſee at all in open day, without knitting their eyebrows, and keeping the eyelids almoſt ſhut. In many of theſe inſtances there is an univerſal glare of light from the pupil, tinged with a ſhade of red; which colour, moſt probably, ariſes from the blood in the veſſels of the choroid coat. I have likewiſe obſerved that the pigmentum is thinneſt when it is light; ſo that ſome of the light, which is reflected from the point of viſion, would ſeem to be thrown all over the inner ſurface of the eye; which being white, or rather a reddiſh white, the light appears to be

again reflected from ſide to ſide*. This ſeemed to be the caſe in a boy at Shepperton, when about three years of age, of whom I have a portrait, to ſhow that appearance. He is now about thirteen years of age†: the common light of the day is ſtill too much for him; the twilight is leſs offenſive. When in a room, he turns his eyes from the window; and when made to expoſe his face to the light, or when out in the open air, he knits his eyebrows, half ſhuts his eyelids, and bends his head forwards, or a little down: yet the light appears to be leſs obnoxious to him now than formerly, probably from habit. Such perſons appear to be nearer ſighted than people in common; but I apprehend that appearance to ariſe from the poſition into which they throw the eyelids and brows, which not only in a great degree excludes the light, making the object faint in proportion to the contraction of the pupil and ſhade made by the eyelids and eyebrows; but at the ſame time fits the eye to ſee near objects: for if we nearly cloſe our eyelids, and knit our eyebrows, we can ſee a ſmall object much nearer than if we did not perform ſuch actions; and it will make above a foot difference in the focal diſtance of the eye.

In many rabbits who have white eyelaſhes, and in white mice, the pigmentum is entirely white; which is likewiſe obſervable in a certain diſtinct ſpecies of animals, the ferret, which we have adduced as an example of the pigmentum being naturally white: for theſe animals being intended to ſee in the dark, and their mode of life not expoſing them to the light, they are liable to be affected by ſtrong light, to a greater degree, than many others.

If it is allowed as probable that, in animals having the pigmentum diverſified, the object to be viewed is thrown upon the lighter coloured portion; how does it happen that ſuch are able to bear the light better than thoſe who have the pigmentum altogether of a light colour? Perhaps it is not the illuminated object itſelf that is offenſive to the retina; but

* How far this is really the caſe, I do not abſolutely ſay. For whatever light comes through the pupil, muſt be reflected from the point of viſion; but I imagined I ſaw the light paſſing through the ſubſtance of the iris.

† The period here alluded to is 1786, when the firſt edition of this work was publiſhed.

that diffufion of light in the one kind of eye, which does not happen in the other.

Having ftated the facts, and the general effect arifing from the diverfified pigmentum, let us next confider the manner in which it is brought about, that fuch animals fee better with little light, than thofe which have the pigmentum wholly black.

Let us then fuppofe the retina to be the organ of fight; and that by the rays which fall upon it being properly refracted, it gives or conveys to the mind an idea of a diftinct object, correfponding with the fenfation of touch. This is the moft common and fimple manner in which vifion is performed, and is that mode which takes place where the pigmentum is black, or nearly fo; and where the greateft quantity of external light is required.

The retina, although fomewhat opaque, is yet fo tranfparent as to allow a confiderable quantity of light to pafs through it. For if this was not the cafe, there could not be thofe differences in the appearance of the eye which I have been defcribing. The rays which pafs through, we may fuppofe, do or do not give fenfation in their paffage; and we may alfo fuppofe, that only thofe which ftrike againft the retina are the caufe of fenfation: but this is not the prefent inquiry; the rays which pafs through the retina, are what I am alone to confider; which falling upon the pigmentum, are there difpofed of according to the reflecting powers of that fubftance. If the pigmentum is black, the rays will then be abforbed and entirely loft; therefore in fuch eyes, vifion can receive no affiftance from it; and confequently a confiderable quantity of light is required to produce diftinct vifion: but in thofe who have fome part of this pigmentum white, we find that the rays of light which pafs through the retina, are reflected back again; and in this cafe it is not unnatural to fuppofe that the reflected rays, in their paffage back, will ftrike againft the retina and increafe the power of vifion. It is evident that a confiderable portion paffes forwards through the retira, which, I fufpect, is partly loft on the inner furface of the lateral and forepart of the eye, where the pigmentum is black, while the remainder paffing

through the pupil, is again thrown on the object looked at. The next thing to be confidered is, whether the fhape of the eye is fuch, as will throw the rays, which paffed through the retina, back upon that membrane, in the fame or nearly in the fame place as that through which they originally came. The eye being a fphere, or approaching to that figure, makes it probable; but whether the curve is fuch as will reflect the rays exactly in the fame direction, is not fo eafily determined. If the curve be a true one, then the rays that are not obftructed in their return by the retina, muft pafs forwards through the pupil; and being refracted in their paffage through the chryftalline humour, will be fent out of the eye in the fame lines in which they entered, and be thrown on the very object from whence they came; which feems to be in a great meafure the cafe, if we may judge by the degree of illumination in the cat's eyes. If the rays, reflected from the light part of the pigmentum, fhould not, in their return, ftrike exactly on the fame points in the retina, through which they firft paffed; yet if they are thrown nearly on the fame place it will be fufficient; for we know that our fenfations are not capable of conveying to the mind mathematical exactnefs. And the fame circumftance will be a fufficient anfwer, fhould it be objected that the time loft in the paffing and repaffing of the rays may prevent diftinct vifion; for it is known, that if an illumined body is made to move quickly in a circle, it will appear to the eye a circle of fire.

THE USE OF THE OBLIQUE MUSCLES.

Muscles are the active parts in an animal body, producing different effects, according to the circumftances in which they are placed; and the greater number of parts requiring a variety of motions, it became neceffary to have a variety of mufcles fuited to fuch motions.

The function of a mufcle depends on the contraction of its fibres; and the moft general effect produced by this contraction, is to move fome one part of the body upon another. But we may obferve, that when motion,

in a part, is performed by one ſet of muſcles, there are other muſcles employed in regulating that motion, as in moſt joints: and in a whole part, deſtined to a variety of motions, and compoſed of ſmaller parts, intended likewiſe to have their diſtinct motions, we find muſcles appropriated for the purpoſe of keeping ſome of thoſe parts fixed in a particular poſition, while the whole part is to be moved by other muſcles, according to the nature of the action to be performed. This will, perhaps, be beſt illuſtrated by attending to what takes place in the eye, conſidering it as part of the head.

The eye being an organ of ſenſe, which is to receive impreſſions from without, it was neceſſary it ſhould be able to give its motions that kind of direction as would permit its being impreſſed by objects whether at reſt or in motion, or moving from object to object; and it was alſo neceſſary, that there ſhould be a power capable of keeping the eye fixed upon an object when our body or head was in motion.

For the better underſtanding this action of pointing the eye towards objects under the various circumſtances of viſion, it will be neceſſary to mention, that the eye is furniſhed with muſcles, ſome of which in the quadruped, bird, amphibia, and fiſhes, are called ſtraight, from their being placed in the direction of, or parallel to, the axis of the eye: and two, I believe, have always been named oblique. Of the ſtraight, ſome animals have more than others. There are four ſtraight muſcles common to moſt animals; and thoſe which have more, have the additional muſcles inſerted immediately in the eyeball, on its poſterior ſurface, and ſurrounding the optic nerve. The four ſtraight muſcles, which are common to all quadrupeds, paſs further forwards, and are rather inſerted towards the anterior ſurface of the eye. For viſion, at large, it was not only neceſſary that the eye ſhould be capable of moving from object to object, or of following any object in motion, but alſo neceſſary that there ſhould be a power to keep it fixed on any one object to which the mind might be attentive; therefore the muſcles are formed ſo as not only to be able to move the eye from object to object, but likewiſe to keep its point of viſion fixed upon any particular one, while the eye is moving progreſ-

ſively with the head or body. This is the uſe of theſe muſcles, when the parts from whence they ariſe are kept fixed reſpecting the objects the eye is pointed to; but it is often neceſſary, while the eye is fixed upon a particular object, that the eyeball and the head in which it is fixed ſhould ſhift their ſituation reſpecting that object; and this would alter the direction of the eye, if the muſcles had not the power of taking up an action that produces a contrary effect, that is, keeping the point of inſertion of the muſcles as the fixed point, by cauſing their fibres to contract according as the origins of the muſcles vary their poſition reſpecting the object. From this mechaniſm we find theſe three modes of action produced; firſt, the eye moving from one fixed object to another; then the eye moving along with an object in motion; and laſt, the eye keeping its axis to an object, although the whole eye, and the head, of which it makes a part, are in motion. From either of theſe motions taking place ſingly, or being combined, the eye is always kept towards its object. In the two firſt modes of action, the origins of the muſcles are fixed points reſpecting the object; and, in the laſt, the object becomes as it were the centre of motion, or fixed point, commanding the direction of the actions of the eye, as the north demands the direction of the needle, let the box in which it is placed be moved in what direction it may. Theſe two firſt modes of action are performed by the ſtraight muſcles; for the head being a fixed point, they are capable of moving the eye up and down, from right to left, with all the intermediate motions, which, taken together, conſtitute a circular movement; or, when the eye is to become the fixed point, then the head itſelf performs the circular movement. Thence appears the neceſſity why the object, the axis of the eye, and the point of ſenſation, ſhould all three be in the ſame ſtraight line. But this does not take place in all movements of that whole of which the eye makes a part; for beſides thoſe which we have already taken notice of, the head is capable of a motion from ſhoulder to ſhoulder, the axis of which is through the axis of the two eyes, from the fore to the backpart. It ſhould be here obſerved, that for diſtinct viſion, the object muſt be fixed as reſpecting the pupil of the eye, and not in the leaſt allowed to move

over its ſurface*. To prevent any progreſſive motion of the object over the retina of the eye, either from the motion of the object itſelf, or of the head in ſome of the motions of that part, the ſtraight muſcles are provided as has been explained; but the effects which would ariſe from ſome other motion of the head, as from ſhoulder to ſhoulder, cannot be corrected by the action of the ſtraight muſcles, therefore the oblique muſcles are provided. Thus when we look at an object, and at the ſame time move our head to either ſhoulder, it is moving in the arch of a circle whoſe centre is the neck; and of courſe the eyes would have the ſame quantity of motion on this axis, if the oblique muſcles did not fix them upon the object. When the head is moved towards the right-ſhoulder, the ſuperior oblique muſcle of the right-ſide acts and keeps the right-eye fixed on the object; and a ſimilar effect is produced upon the left-eye by the action of its inferior oblique muſcle: when the head moves in a contrary direction, the other oblique muſcles produce the ſame effect. This motion of the head may, however, be to a greater extent than can be counteracted by the action of the oblique muſcles. Thus, for inſtance, while the head is on the left-ſhoulder, the eyes may be fixed upon an object, and continue looking at it while the head is moved to the right-ſhoulder, which ſweep of the head produces a greater effect upon the eyeballs than can be counteracted by the action of the oblique muſcles; and in this caſe we find that the oblique muſcles let go the eye, ſo that it immediately returns into its natural ſituation in the orbit. Whether

* Optical writers ſeem to have been entirely ignorant of this; for they not only ſuppoſe diſtinct viſion compatible with the object having a motion over the different parts of the retina, but even explain the effects which would be produced by it on the mind of the obſerver. Keill makes the following obſervation:

"Since opticks teach us, that every body which is viſible, has by means of the rays which proceed from that object, its image painted on the bottom of the eye, or retina; it follows, that thoſe objects will ſeem to be moved, whoſe images are moved on the retina, that is, which paſs over ſucceſſively the different parts of the retina, whilſt the eye is ſuppoſed to be at reſt: but thoſe objects will be looked upon as being at reſt whoſe images always occupy the ſame part of the retina, that is, when the motion of thoſe images are not perceived in the bottom of the eye." Keill's Introduction to Natural Philoſophy, page 79.

this is performed by the natural elaſticity of the parts; or, whether the antagoniſt oblique muſcles take up the action and reinſtate the eye, I do not know. If the head ſtill continues its motion in the ſame direction, then the ſame oblique muſcles begin to act anew, and go on acting, ſo as to keep the eyes fixed on the object. As this motion of the head ſeldom takes place uncombined with its other motions, ſome of the ſtraight and oblique muſcles will be employed at the ſame time, according as the motions are more or leſs compounded.

A DESCRIPTION OF THE NERVES WHICH SUPPLY THE ORGAN OF SMELLING.

THE nerves being in themſelves perhaps the moſt difficult parts of an animal body to diſſect, becomes a reaſon why we are ſtill unacquainted with many of their minuter ramifications: yet if a knowledge of theſe, together with that of their origin, union and reunion, is at all connected with their phyſiology, the more accurately they are inveſtigated, the more perfectly will the functions of the nerves be underſtood. I have no doubt, if their phyſiology was ſufficiently known, but we ſhould find the diſtribution and complication of nerves ſo immediately connected with their particular uſes, as readily to explain many of thoſe peculiarities for which it is now ſo difficult to account. What naturally leads to this opinion is, the origins and number of nerves being conſtantly the ſame; and particular nerves being invariably deſtined for particular parts, of which the fourth and ſixth pair of nerves are remarkable inſtances. We may therefore reaſonably conclude, that to every part is allotted its particular branch; and that however complicated the diſtribution may be, the complication is always regular. There are ſome nerves which have a peculiarity in their courſe, as the recurrent and chorda tympani; and others which are appropriated to particular ſenſations, as thoſe which go to four of the organs of ſenſe, ſeeing, hearing, ſmelling, and taſting; and ſome parts of the body having peculiar ſenſations, (as the ſtomach and penis) we may, without impropriety, include the fifth, or ſenſe of feeling. This general uniformity, in courſe, connection and diſtribution, will lead us to ſuppoſe that there may be ſome other purpoſe to be anſwered than mere mechanical convenience; and many of the variations which have been deſcribed in the diſſections of nerves, I believe to have ariſen from the blunders of the anatomiſt, rather than from any irregularity in

their number, mode of ramifying, courſe, diſtribution, or connection* with each other. We obſerve no ſuch uniformity in veſſels carrying fluids; but find particular purpoſes anſwered by varying their origin and diſtribution: the pulmonary artery anſwers a very different purpoſe in the circulation of the blood, from that of the aorta; yet both ariſe from the ſame ſource, the heart. The courſe of the arteries is ſuch as will convey the blood moſt conveniently, and therefore not neceſſarily uniform; it not being very material by what channel, provided the blood is conveyed to the part; though, in particular inſtances, certain purpoſes may be anſwered by a peculiarity in origin and diſtribution, as happens in the teſticle of quadrupeds. This obſervation reſpecting arteries is likewiſe applicable to veins, and ſtill more to the abſorbent veſſels, in which laſt, regularity is even leſs eſſential than in the veins. Whoever, therefore, diſcovers a new artery, vein, or lymphatic, adds little to the ſtock of phyſiological knowledge; but he who diſcovers a new nerve, or furniſhes a more accurate deſcription of the diſtribution of thoſe already known, affords us information in thoſe points which are moſt likely to lead to an accurate knowledge of the nervous ſyſtem: for if we conſider how various are the origins of the nerves, although all ariſe from the brain, and how different the circumſtances attending them, we muſt ſuppoſe a variety of uſes to ariſe out of every peculiarity of ſtructure. Indeed, if we reflect on the actions ariſing immediately from the will, and affections of the mind, we muſt ſee that the origin, connection, and diſtribution of the nerves, ought to be exact, as there are parts whoſe actions immediately depend upon ſuch circumſtances. The brain may be conſidered as having an intelligence with the body; but no ſuch intercourſe ſubſiſts between the different parts of the body and the heart.

* Here it is to be underſtood I do not mean lateral connection; ſuch as two branches uniting into one chord and then dividing; or a branch going to a part, either ſingle or double, for ſtill it is the ſame nerve; or whether a branch unites with another a little ſooner or a little later, for ſtill it is the ſame branch. Such effects may ariſe more from a variety in the ſhape of the bodies they belong to, than any variety in the nerves themſelves.

In the ſummer of 1754, being much employed in diſſecting the nerves paſſing out of the ſkull; I was, of courſe, led to trace many of their connections with thoſe from the medulla ſpinalis; and was aſſiſted by Dr. Smith, then purſuing his ſtudies in London*. The better to trace theſe nerves through the foramina of the ſkull, I ſteeped the head in a weakned acid of ſea-ſalt till the bones were rendered ſoft, and that the parts might be as firm as poſſible, and at the ſame time free from any tendency to putrefaction, (it being ſummer) the acid was not diluted with water, but with ſpirit. When the bones were rendered ſoft, purſuing my intention, I diſſected the firſt pair of nerves, and diſcovered their diſtribution; and having made a preparation of the parts in which they were found, I immediately had drawings made from them, with a view to have preſented the account to the Royal Society, but other purſuits prevented it†. Engravings were afterwards made from theſe drawings; and the preparation was repeatedly ſhown by Dr. Hunter, in his courſes of anatomy, who, at the ſame time, pointed out that alteration in the mode of reaſoning upon thoſe nerves, which would naturally ariſe from this diſcovery. In this diſſection I found ſeveral nerves, principally from the fifth pair, going to and loſt upon the membrane of the noſe; but ſuppoſe that thoſe have nothing to do with the ſenſe of ſmelling; it being more than probable, that what may be called organs of ſenſe, have particular nerves, whoſe mode of action is different from that of nerves producing common

* Dr. Smith was afterwards teacher in chymiſtry and anatomy, in the univerſity of Oxford; is now Savilian profeſſor of geometry, and lecturer in phyſiology. This account of the firſt pair of nerves, as alſo of the branches of the fifth, is taken from the original deſcription written by him, aud taken from my diſſection when I was tracing them.

† Dr. Scarpa, profeſſor of anatomy at Pavia, while in London, in 1782, acquainted me that he had diſſected the ramifications of the olfactory nerves; and that on his return to Italy he meant to publiſh an account of them. At this time I ſhewed him my drawings and engravings. I have lately been informed that he has publiſhed his account, but have not met with it: I have, however, ſeen one of his engravings, which was executed in London, and is very elegant. It only ſhews thoſe on the ſeptum narium, whoſe minuteneſs is rather carried further than the power of diſſection, and the ramifications are more regular than we find them in Nature.

ſenſation; and alſo different from one another; and that the nerves on which the peculiar functions of each of the organs of ſenſe depend, are not ſupplied from different parts of the brain. The organ of ſight has its peculiar nerve; ſo has that of hearing; and probably that of ſmelling likewiſe; and, on the ſame principle, we may ſuppoſe the organ of taſte to have a peculiar nerve. Although theſe organs of ſenſe may likewiſe have nerves from different parts of the brain; yet it is moſt probable ſuch nerves are only for the common ſenſations of the part, and other purpoſes anſwered by nerves. Thus we find nerves from different origins going to the parts compoſing the organ of ſight, which are not at all concerned in the immediate act of viſion; it is alſo probable, although not ſo demonſtrable, that the parts compoſing the ear have nerves belonging to them ſimply as a part of the body, and not as the organ of a particular ſenſe: and if we carry this analogy to the noſe, we ſhall find a nerve, which we may call the peculiar nerve of that ſenſe; and the other nerves of this part, derived from other origins, only conveying common ſenſation, and we may ſuppoſe only intended for the common actions of the part. This mode of reaſoning is equally applicable to the organ of taſte; and if the opinion of peculiar nerves going to particular organs of ſenſe, be well founded, then the reaſon is evident why the noſe, as a part of our body, ſhould have nerves in common with other parts, beſides its peculiar nerves; and, as the membrane of the noſe is of conſiderable extent, and has a great deal of common ſenſation, we may ſuppoſe the nerves ſent to this part, for that purpoſe, will not be few in number. It is upon this principle the fifth pair of nerves may be ſuppoſed to ſupply the eye and noſe in common with other parts; and, upon the ſame principle, it is more than probable, that every nerve ſo affected as to communicate ſenſation, in whatever part of the nerve the impreſſion is made, always gives the ſame ſenſation as if affected at the common ſeat of the ſenſation of that particular nerve*.

* I knew a gentleman who had the nerves which go to the glans penis completely deſtroyed by mortification, almoſt as high as the union of the penis with the pubes; and at the edge of the old ſkin, at the root of the penis, where the nerves terminated, was the peculiar ſenſation

The firſt pair of nerves arriving at the part of its deſtination as ſoon as it eſcapes from the ſkull, and immediately ramifying, has rendered its diſtribution more obſcure than that of the others, whoſe courſe to the part to which they are allotted is viſible and to be traced. As the body of the nerve, while within the ſkull, is pulpy and compoſed of the brain itſelf, it eaſily breaks off at the very diviſion and exit of the ſmall branches; it therefore becomes impoſſible to trace them, as we uſually do other nerves; and they have by moſt phyſiologiſts been conſidered as never forming chords, but going on in their pulpy form to be diſtributed on the membrane of the noſe, in a mode ſomewhat ſimilar to that of the optic nerve; and to what is commonly ſuppoſed to take place with reſpect to the portio mollis of the ſeventh pair. Winſlow has ſuggeſted an idea, that the firſt pair forms chords; but it is only as an aſſertion; and not having deſcribed them, that alone was not ſufficient to alter the former mode of reaſoning.

Haller, who is to be conſidered as the lateſt anatomiſt and phyſiologiſt, who has publiſhed on the ſubject, on whom we can depend, ſays, "That the firſt pair of nerves makes its way into the noſe, covered by the pia mater only, very little altered from what it was when within the cavity of the ſkull*." This ſhows that Haller retained the old idea concerning theſe nerves: but we ſhall find that they become firm chords immediately upon piercing the dura mater and cribriform plate of the ethmoid bone.

The firſt pair, while within the ſkull, differs in ſome reſpects from all other nerves; firſtly, it ſeems to be made up of a cortical and medullary

of the glans penis; and the ſenſation of the glans itſelf was now only common ſenſation; therefore the glans has, probably, different nerves, and thoſe for common ſenſation may come through the body of the penis to the glans.

A ſerjeant of marines who had loſt the glans, and the greater part of the body of the penis, upon being aſked, if he ever felt thoſe ſenſations which are peculiar to the glans, declared, that upon rubbing the end of the ſtump, it gave him exactly the ſenſation which friction upon the glans produced, and was followed by an emiſſion of the ſemen.

* Elementa Phyſiologiæ, vol. 5. page 151.

ſubſtance, while the others appear to conſiſt of medullary alone; and ſecondly, it is different, in that it does not ſeem to be compoſed of faſciculi, and has but one covering from the pia mater inveſting the whole nerve; whereas other nerves appear to have a covering round each faſciculus; and this is probably the reaſon why the firſt pair is weaker while within the ſkull, than the others. Its form is ſomewhat triangular, having three edges, from lying in a groove, made by two convolutions of the brain. The courſe is forwards, a little upwards and inwards, and where it lies upon the cribriform plate of the ethmoid bone, becomes ſomewhat larger and divides into a great many branches, like ſo many roots, anſwering to the number of holes in that plate, except one left for a branch of the fifth pair; but theſe diviſions we cannot ſee, they being covered by the body of the nerve, which cannot be raiſed without breaking off the ſmall branches at their origin. As the branches of the nerve paſs through this bone, they ſeem to take proceſſes from the dura mater along with them, then becoming firm chords, ſimilar to other nerves. Theſe branches, after they have got through the bone, form themſelves into two planes or diviſions, one paſſing on the ſeptum, the other on the turbinated bones. Thoſe of the ſeptum narium, in their paſſage to the noſe, are firſt continued a little way down, in bony canals of the perpendicular lamella of the ethmoid bone, which holes become ſmall grooves in that bone; and thoſe on the oppoſite ſide being more numerous and ſmaller, paſs down through ſmall holes that are on the inſide plate of the ethmoid bone, which holes are likewiſe continued into grooves, for a little way, upon that plate. When the branches get upon the membrane of the noſe, they ſubdivide into a great many ſmaller ones, which are ſomewhat flattened, and are only to be ſeen on that ſide of the membrane that adheres to the bones, not being viſible at all on the other; ſo that the diſſection of theſe nerves is no more than ſeparating the membrane and bone from each other. They can hardly be diſſected all round; and the further they are traced upon the membrane the fainter they become, and growing ſmaller, they ſink deeper and deeper into the membrane to get on its outer ſurface, where we muſt ſuppoſe they terminate. Thoſe upon

the ſeptum paſs down a little radiated, and the branches, eſpecially at the upper part, or at their firſt ſetting out, unite with one another. Thoſe on the ſide next the antrum, when they have reached the membrane of the noſe, in their courſe to the ſuperior turbinated bone, form a very conſiderable network or plexus; and when they reach that bone, do not all go round its convex curvated pendulous edge to the concave ſide; but ſome paſſing through its ſubſtance, get immediately upon it; which is the reaſon why we find ſo many holes in that bone. It is difficult to trace them further; but we have reaſon to ſuppoſe that they go through the inferior turbinated bone in the ſame manner, ſince we find ſimilar holes.

A DESCRIPTION OF SOME BRANCHES OF THE FIFTH PAIR OF NERVES.

In tracing the courſe of the olfactory nerves, I alſo diſcovered ſeveral branches of the fifth pair, not commonly known, particularly two that were ſuppoſed to go to the membrane of the noſe for the ſenſe of ſmelling; but which only paſs through that organ to their place of deſtination. The firſt is a ſmall nerve from the firſt branch of the fifth pair; or, according to Winſlow, the nervus ophthalmicus Williſii; which ſmall nerve is called by Winſlow, the naſal. This branch, after having paſſed out of the ſkull into the orbit, re-enters the cranium through the foramen orbitarium anterius, and gets on the cribriform plate of the ethmoid bone; from thence it paſſes down through one of the anterior holes of the cribriform plate, and after having continued its courſe in a groove on the naſal proceſs of the frontal bone, runs forward and downward in a ſimilar groove on the inſide of the os naſi; from thence getting on the outſide of the cavity of the noſe, it runs along the cartilaginous part of the ala, and near the extremity of the noſe mounts up upon the tip of the ala, and then dipping down between the two alæ, is loſt on the anterior extremity of the cartilaginous ſeptum. In its courſe it ſends ſeveral ſmall filaments into the alæ.

The ſecond, is a branch of the ſuperior maxillary nerve; for that nerve having paſſed through the foramen rotundum, divides and ſends off ſeveral branches, one of which paſſing backwards and inwards, through the foramen commune, between the orbitar proceſs of the palate, and the root of the ala of the ſphenoid bone, gives a branch which gets into a fiſſure that ſeems to ſeparate the root of the ala from the body of the ſphenoid bone, where that bone makes the roof of the noſe. This branch then paſſes along the under ſurface of the body of the ſphenoid bone, in its way to the ſeptum narium, and getting upon that part, paſſes along between its membranes and the bone: its courſe is downwards, and forwards towards the foramen inciſivum, through which it paſſes and is loſt in the gum behind the firſt dentes inciſores, and on the membrane of the roof of the mouth at that part.

There is another branch of the ſuperior maxillary nerve, which comes off from a large branch that is going down to the mouth uvula, &c. and this branch, with its diviſion into two, has been deſcribed by profeſſor Meckel of Berlin; but after tracing one of theſe into the portio dura, he purſued the ſearch no further. This branch of the ſuperior maxillary nerve paſſes back through the foramen pterigoideum, accompanies the carotid artery as it paſſes acroſs the poſterior edge of the foramen, and there divides into two branches; one of which paſſes down along with the carotid artery, through the baſis of the ſkull, and proceeding in a direction contrary to the courſe of the artery, in contact with that branch of the cervical ganglion that paſſes up with the carotid artery to join the ſixth pair; then joins the firſt cervical ganglion. The other branch decuſſates that artery on its upper ſurface, and getting upon the anterior ſide of the petrous portion of the temporal bone, enters a ſmall hole near the bottom of that large one which affords a paſſage to the ſeventh pair of nerves, joining the portio dura, juſt where that nerve making its firſt turn, paſſes along with it through what is called the aqueduct. This nerve, compoſed of portio dura, and the branch of the fifth pair, ſends off, in the adult, the chorda tympani before its exit from the

ſkull; and in the fœtus, immediately after. The termination of the branch, called chorda tympani, I ſhall not deſcribe; yet I am almoſt certain it is not a branch of the ſeventh pair of nerves, but the laſt-deſcribed branch from the fifth pair; for I think I have been able to ſeparate this branch from the portio dura, and have found it lead to the chorda tympani; perhaps, is continued into it; but this is a point very difficult to determine, as the portio dura is a compact nerve, and not ſo faſciculated as ſome others are; however this may be, it is very reaſonable to ſuppoſe that the chorda tympani is a branch of the fifth pair, as it goes to join another branch ariſing from the ſame trunk.

Plate XVII.

J. V. Reimsdyk del.

W. Sharp sculp.

PLATE XVII.

THE olfactory, or firſt pair of nerves, as they are ſeen upon the membrane of the ſeptum narium.

The bony ſeptum is removed to expoſe the nerves of the right noſtril, as they paſs at firſt between the membrane and bone.

A The os frontis.
B The frontal ſinus.
C The cartilaginous part of the ſeptum narium.
**** The cut edge, from which the ſeptum has been ſeparated all round.
D The ſurface of the common ſkin, where it is loſt in the membrane of the noſe.
E The upper lip.
F Part of the alveolar proceſs of the maxillary bone next the ſymphyſis.
G The roof of the mouth.
H The bony palate.
I The uvula, and palatum molle.
K The upper part of the fauces,
L The opening of the Euſtachian tube.
M The cuneiform proceſs of the os occipitis.
N The inſide of the cuneiform proceſs, near the foramen magnum occipitale.
O The poſterior clinoid proceſs.
P The ſphenoid ſinus, with its ſeptum.
Q The ſella Turcica.
R The criſta galli.
SS The membrane of the right noſtril that lined the ſeptum; the ſeptum being removed.
T A branch of the fifth pair of nerves, that comes through the foramen commune, or ſpheno-palatinum.
UUU The firſt pair of nerves, having paſſed through the cribriform plate of the ethmoid bone, ramifying on the membrane of the ſeptum.

Plate XVIII

J. V. Reimsdyk del.

W. Sharp sculp.

PLATE XVIII.

THE olfactory, or firſt pair of nerves, as they are ſeen upon the membrane of the noſe, which covers the turbinated bones; the exterior parts of the face being removed.

This engraving was taken from the ſame head as plate XVII.

A The os frontis.
B The os naſi.
C The cartilaginous and membranous part of the noſe.
D The ala naſi, with the ſkin left on.
E The ſeptum narium.
F The upper lip.
HHH The alveolar proceſs of the ſuperior maxillary bone.
I Part of the antrum.
K The os occipitis.
L The body of the ſphenoid bone.
M The groove made by the carotid artery.
N The poſterior clinoid proceſs.
O The ſphenoid ſinus.
P The criſta galli.
Q The membrane of the noſe.
R The membrane, a little more convex, where the inferior turbinated bone is ſituated.
S The ſame where the ſuperior turbinated bone is ſituated.
T The branch of the fifth pair of nerves that was ſuppoſed to be loſt on the membrane of the noſe.
UUU The trunk of the firſt pair of nerves which is afterwards loſt upon that part of Sneider's membrane that covers the turbinated bones.

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